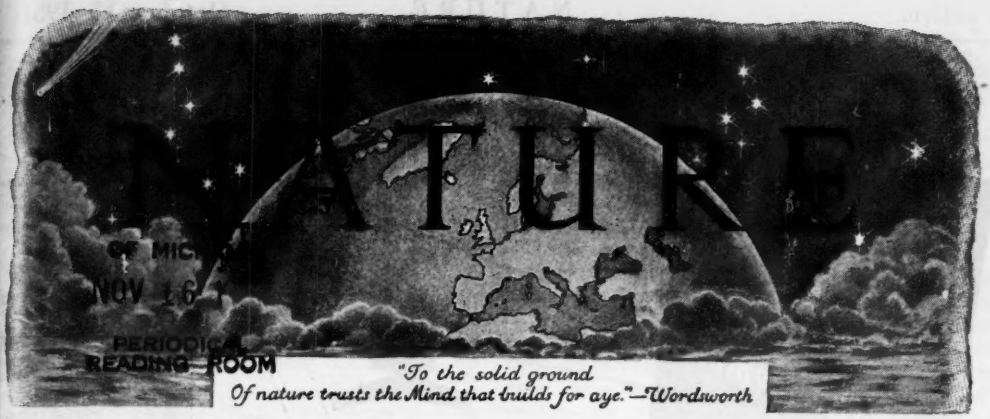

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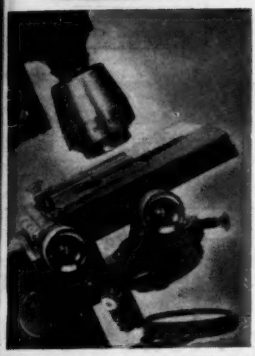

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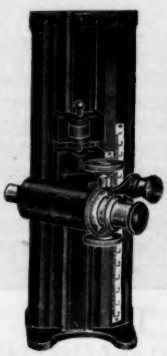

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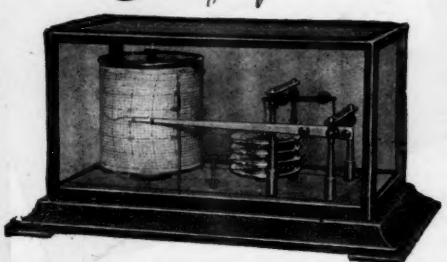

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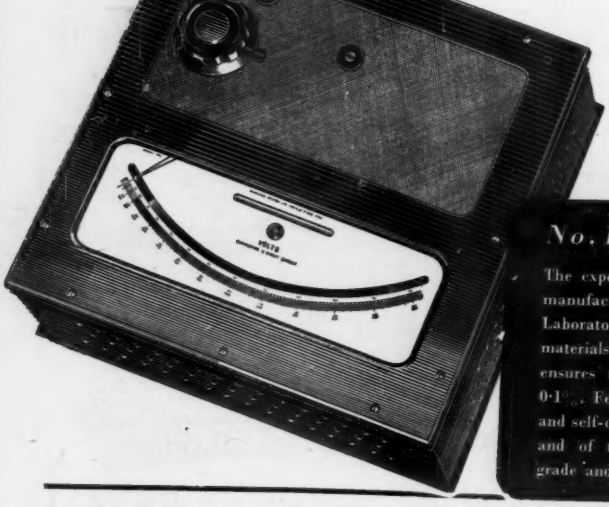

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THE BALANCE OF SCIENTIFIC RESEARCH AND DEVELOPMENT

A PASSAGE in the latest report of the Advisory Council on Scientific Policy gives point to misgivings recently expressed as to the possible restriction of national expenditure on scientific research and development. The Advisory Council is already considering the extent to which a policy of restricting such expenditure could safely be pursued; indeed, quite apart from the effect of rising costs, in present circumstances it is clearly imperative that those responsible for the conduct of research in all institutions dependent, like the universities, to any extent upon State support, should look carefully at their programmes with the view of making sure that resources are not being wasted. In a recent press interview in London, Dr. C. A. Thomas, chairman of the Board of Directors of the American Chemical Society, expressed concern as to whether in Britain and in some Continental countries fundamental research is prosecuted to-day as intensively as in the past. Dr. Thomas paid tribute to British excellence in this field and suggested that unless our effort is maintained the Western powers will be seriously handicapped in future.

Dr. Thomas's concern appeared to be prompted at least in part by the fear that political conditions such as nationalization and government monopoly might be inimical to the atmosphere in which fundamental research can flourish, and that bureaucracy might lead to industrial stagnation or atrophy. Whether those fears are well-founded or not—and reports from the Nuffield Foundation suggest they warrant examination—they are doubtless also inspired by the emphasis placed in Great Britain in recent years on our shortcomings in applied research. Sir Henry Tizard's remarks in his presidential address to the British Association in 1948, that the productivity of labour in Britain is far lower than it would be if the results of past research were more resolutely and continuously applied, and his view that it is not the general expansion of research that is of the first importance for the restoration of industrial health, have been widely quoted. It is generally recognized that that industrial prosperity will depend more and more on the continuous application of science to industrial practice. Unless we can raise our standard of technology, unless there are many more men in executive positions in industry whose practical experience has been preceded by a scientific education, we shall inevitably fail to keep our place among the great manufacturing nations.

In the long run, as Sir Henry then pointed out, the expansion of research will fail of its full effect unless at the same time we promote education in the higher branches of technology and unless the technologist takes his rightful place in industry. Sir Ewart Smith's address last year at the Birmingham meeting of the British Association on what was essentially this text made a great impression, and although little has been done in the past three years to implement the expansion of higher technological education, the

concentration on this issue may well have suggested to Dr. Thomas that fundamental research in Great Britain is likely to be curtailed so as to permit the expansion of applied or industrial research. Such a view is not, however, supported by the trend of the reports of the Advisory Council on Scientific Policy, in spite of the stress laid on securing the fuller and wider application of existing knowledge. It is widely recognized that the prosecution of fundamental research on an adequate scale and under appropriate conditions is an essential factor in the well-being of every industrial nation, and one which cannot be neglected with impunity. Even when writing under the title "The Neglect of Science", Prof. F. E. Simon, in his recently reprinted essays to laymen, does not suggest that the position in Britain is unsatisfactory, but that the need is rather to ensure that our opportunities and resources for scientific research are fully used.

It is, of course, well to recognize, as Lord Hankey pointed out on August 29 in his Centenary Lecture, under the auspices of the Ministry of Labour and National Service (available from the Ministry), on higher technical and scientific man-power, that when government expenditure on science and technology in its own establishments, universities, technical colleges and the like, reaches its present high level, there is latent danger of starving pure science through too great insistence on immediate and practical results. He did not think that this has happened, or is likely to happen in Britain in the near future, under the present system, but universities must be on guard against the risk. It is one to be watched all the more carefully at a time of financial stringency, when some halt in expenditure, if not actual reduction, may be unavoidable; and the risk is one to be considered in determining the form which the expansion of technological education should take.

As to the necessity for such expansion, there is general agreement, in spite of the competition for resources which the rearmament drive and the deterioration of the general economic situation in the past year or so may involve. Concern to improve the means or even the resources available to secure the application of existing knowledge has not in practice led to proposals to direct resources from the prosecution of scientific research to the application of its results. On the contrary, it is recognized that considerable expansion in both higher technological education and in technical education is imperative. The real questions at issue are the relative scale on which such expansion should take place, and where and how it can most efficiently be achieved.

On those questions Lord Hankey was able to give little guidance in the Lecture just noted. As chairman of the recently reconstituted Technical Personnel Committee, he could not well discuss the future work of that Committee; but its reconstitution should ensure that expansion of technological education will be on the basis of quantitative figures for the supply and demand of technologists and technicians.

It is from this point of view that Prof. A. R. Ubbelohde's contribution to a symposium of articles on postgraduate studies in the *Universities Quarterly* of August is of special interest at the present time. Dealing particularly with scientific research at the universities, Prof. Ubbelohde makes a number of very important points. He recognizes that university policy should seek to foster not only the vitality of individual schools, but also to impart a unifying spirit to all the diverse cells of research and scholarship it embraces; he reminds us, however, first that academic research must maintain its broad contacts with other intellectual activities in the university, and secondly that it should not make inflexible demands which would distort general university provisions for the advancement of knowledge.

These are the considerations which Prof. Ubbelohde applies to the critical review of future university policy in this field. First, almost challenging Sir Cyril Hinshelwood's remark to Section B (Chemistry) at the British Association meeting in Edinburgh, he points out that the failure of the scientific man to take a proper part in university administration may be detrimental to the general educational functions of the natural sciences. Admittedly, it is difficult to find really balanced university teachers of the highest quality; but if scientific men shirk their administrative responsibilities in the university, wasteful and dangerous situations can arise, just as they can when the use of the scientific man in public affairs is neglected.

One consequence in the university to which Prof. Ubbelohde directs attention is the warping of the selection of new staff and the provisions for teaching commitments. On this point he makes several suggestions designed to avoid such distortion and to secure the most effective use of staff for research during the creative years. These may seem relatively minor matters; yet it is only by attention to such administrative detail that we can hope to make the most of our limited resources of man-power, not only in the universities but also in other fields of activity.

Even more important, however, particularly in view of current discussions on higher technological education, are Prof. Ubbelohde's observations on the conduct of very high-cost research in the universities. No really satisfactory location of such research is, in his view, possible within the academic framework, and he urges that research programmes requiring very costly capital equipment should normally be segregated to the appropriate research institute or association. Their location in the universities tends to limit the free choice of research programmes by the universities, leading to the selection of programmes of comparatively poor training value, and limiting the experience of the young research worker as well as the flexibility of the research school around such equipment, and particularly the choice of new senior appointments.

In a technological foundation specially instituted for postgraduate work, mature and adequately paid staff can be engaged to run such high-cost equipment, and a better return of new results can be expected before the equipment becomes obsolete. It should

not be difficult to provide access to such apparatus for the universities when necessary.

Prof. Ubbelohde makes the further suggestion that the separation of technologies from the academic community should be made at the postgraduate stage, when really heavy costs of equipment first arise. The risk of advanced work and research at the universities being crowded out by demands of expensive equipment requiring administration of an entirely different type would thus be avoided.

One point on which there is general agreement at the present time is that, even in face of the rearmament drive, much more must be spent on technical education, including higher technological education. It is all the more important, therefore, not only that we should, as the Duke of Edinburgh emphasized at Edinburgh, develop a co-ordinated system of scientific and technological education, lack of which is a prime cause of our failure to produce sufficient trained technologists to meet the urgent needs of scientific development in industry and to provide leaders for the future, but also that we should ensure that the money appropriated is most effectively spent. The policy to be followed should ensure so far as possible that all sources of wasted man-power or equipment such as those to which Prof. Ubbelohde directs attention are avoided.

No such policy can be achieved if excessive tenderness is shown to the prejudices or wishes of individual authorities or institutions : the national interest must prevail over sectional interest, but for reasons given and not by bureaucratic decree. It must be recognized that many parties concerned have already made contributions towards the delineation of the principles on which an agreed policy could be based. At Edinburgh, for example, Prof. J. C. Prescott indicated clearly principles which should determine the scope of university teaching in technology. The Association of University Teachers, in an appendix to its report on the place of technology in the universities, published last May, gave some figures regarding the extent to which technologies are dealt with in British university institutions; these show that the range covered at the Massachusetts Institute of Technology is individually equalled or excelled by Birmingham, Newcastle, Leeds, Liverpool, the Imperial College of Science and Technology and Manchester. Staff-standard ratios are not a function of the size or character of the institution, and may well be influenced considerably by the number of staff employed on full-time research.

The Association draws the further conclusion that in Great Britain the distribution of students among the subjects accords reasonably with the relative importance of the various industries ; and, when due regard is paid to the location of industry, duplication of subjects among institutions is not excessive. Further, it considers that the number of technological students in British universities could be substantially increased without seriously disturbing the balance of faculties. It also comments on the extent to which the resources of the technological institutes such as those of Zurich and of Massachusetts are devoted to sponsored research. That was the subject of adverse comment in the report of the recent Committee on Educational Survey of the latter Institute ; but there is no reason for supposing that it would seriously warp the work of any such institutions which might be established in Great Britain. Other institutions like the Technical University of Delft are conspicuously free of such influence, and all research is done on their own initiative ; but the danger must be kept in mind in considering the expansion of research in the colleges of technology generally, as suggested by Dr. H. J. Cull.

This note from the Association of University Teachers is exactly the kind of survey which should have come independently from the Advisory Council for Scientific Policy. It illustrates once more the imperative necessity for dealing with the expansion of university and higher technological education as a whole, and for placing it under a single competent authority. Moreover, here again we see how closely the research aspect of technological education, and possibly that of teaching also, is linked up with the work of the research associations. It is manifest that the whole problem is one for which the responsibility in our existing organization for research and development rightly falls on the Lord President of the Council, to whom indeed, through the Advisory Council on Scientific Policy, the new Standing Committee on Scientific Man-power is presumably responsible.

The position disclosed by the latest returns from the University Grants Committee for the academic year 1949–50 emphasizes the need for early assessment of the situation as a whole. The expansion of the full-time student population of universities and university colleges of Great Britain to 85,421—the highest ever recorded—as compared with 50,002 in 1938–39, gives no indication of the extent to which the accommodation of the universities is already seriously strained. No significant increase in the student population, the Committee comments, would be possible without the provision of new buildings on a much larger scale than seems practicable in present conditions. This is its reason for regarding any substantial increase in this figure as unlikely in the near future. It might have added that the crowding of lecture rooms gives no idea of the even more serious congestion of the time-tables of the teaching staff. Some halt in expansion must, in fact, be called if standards are to be maintained ; and the increasing extent to which students are dependent on scholarships and other forms of financial assistance might well in present circumstances operate to afford the universities a breathing space for consolidation and long-term planning. Meanwhile, it should be noted that the number of full-time students in technology, 10,933, was relatively unchanged from 1948–49 (10,884), although the number of advanced students of technology, included in these figures, increased from 858 in 1948–49 to 953.

Over the past three years, the number of full-time students of technology in the universities, though slightly increasing, has been relatively stable and is

now only 12·8 per cent of the total as against 12·9 per cent in 1947–48. The essential point is that policy should be formulated, not by the Ministry of Education or any authority concerned with one segment of the problem, but by some authority carrying a general responsibility, independent and strong enough to resist sectional pressure and to probe *ex parte* statements and capable of taking a balanced view of the requirements of Great Britain in regard to the supply of trained men and women, the resources available for the production of that supply, and the repercussions of the redistribution of the effort demanded upon other national needs; this applies not merely in science or technology but also in other fields. Without such a balanced view and wide responsibility, there can be little hope of a policy or programme which will meet the situation without involving waste of resources in man-power and materials that can ill be spared now, and will be hard to recoup by greater efficiency or productivity in the long run.

NATURAL PRODUCTS OF INDIA

The Wealth of India

A Dictionary of Indian Raw Materials and Industrial Products. Vol. 2, Part 1: Raw Materials. Pp. xx+427. Rs. 25; 40*s*. Vol. 2, Part 2: Industrial Products. Pp. xii+251. Rs. 15; 24*s*. (Delhi: Council of Scientific and Industrial Research, 1950–51.)

IN the last two decades of the nineteenth century considerable attention was paid by men of science in India, more especially by Warden and Hooper, to the study of Indian natural products. Much of this important work was summarized by Sir George Watt in his two classics, "A Dictionary of the Economic Products of India" and "The Commercial Products of India". Owing possibly to the rapid development in Europe and elsewhere of synthetic organic chemistry, this interest gradually waned. Only recently has there been a notable revival associated more especially with the names of Seshadri, Siddiqui and Venkataraman.

No doubt the study of Indian natural products will form one of the main subjects for research in the National Chemical Laboratory in Poona, while those plant products which have played so large a part in indigenous medicine will be examined at the Central Drug Research Institute opened last February in Lucknow. On that occasion Sir Edward Mellanby remarked: "It is an astonishing fact that whereas a large part of the plants, from which official drugs in the pharmacopœia are made, are already found in this country, yet many of these drugs are imported in large quantities".

To future research workers in these fields the dictionary "The Wealth of India" will undoubtedly prove of great value. When Parts 1 and 2 of Vol. 1, covering the letters *A* and *B*, were reviewed (see *Nature*, 163, 743; 1949), the view was then expressed that the publication would prove indispensable to all those interested in natural products. This is equally true of the present work, Vol. 2, Part 1, "Raw Materials", and Part 2, "Industrial Products", which have now appeared under the editorship of Mr. B. N. Sastri, who has replaced Mr. B. L. Manjunath.

Part 1, which covers the letter *C*, comprises some four hundred pages and is liberally illustrated with excellent coloured plates and line drawings. It is clearly impossible within the scope of a brief review to refer in detail to the individual articles; but one cannot but be impressed by their high quality, their value being much increased by the numerous references to the original literature. In dealing with agricultural products, not only are details given of the most satisfactory methods for their cultivation, their processing and their chemistry, but also statistics are included of Indian production and exports. In the article on pyrethrins there is an informative table of the effect of this insecticide on the pests attacking Indian crops. In view of the fact that although the spinning mills are in India jute is mainly grown in Pakistan, the lengthy article on this subject is of particular interest, and it is clear that the cultivation of jute in India is being widely extended. It is hoped that this will not be allowed to curtail the growing of foodstuffs in which India is so lamentably deficient. It is natural in a dictionary covering so many subjects that it is not free from errors, and in some cases recent work has been overlooked. This is, however, a very minor defect. The book is beautifully printed and produced, and it is remarkably free from typographical errors. It can be most cordially recommended, and its low price (40*s*.), which must be below the cost of production, should ensure a wide circulation.

Outside India, Part 2, "Industrial Products", will have a much more limited appeal. The articles are very diverse, dealing with such subjects as cotton spinning, clocks, cigars and cigarettes. Perhaps the main interest of this volume outside India will be to indicate how rapidly industrialization is advancing. Naturally there is some overlap between Parts 1 and 2 as, for example, in the articles on camphor; but this is unavoidable. It may be anticipated that this account of Indian industries will act as a catalyst and stimulate the further advance of India as an industrial nation. Like Part 1, Part 2 is well illustrated and its cost (24*s*.) is low.

J. L. Simonsen

CHEMISTRY OF HETEROCYCLIC COMPOUNDS

Heterocyclic Compounds

Edited by Robert C. Elderfield. Vol. 1: Three-, Four-, Five- and Six-Membered Monocyclic Compounds containing One O, N and S Atom. Pp. ix+703. 88*s*. net. Vol. 2: Polycyclic Five- and Six-Membered Compounds containing One O or S Atom. Pp. vii+571. 120*s*. net. (New York: John Wiley and Sons, Inc.; London: Chapman and Hall, Ltd., 1950–51.)

THE need for an up-to-date reference book on the chemistry of organic heterocyclic compounds has been widely recognized for some time, and, from several quarters, indications have been given that the need was to be met. The publication of the first two of a projected series of volumes on the subject is consequently a matter of general interest, and Prof. R. C. Elderfield's work will be received with eagerness and carefully scrutinized.

In form, these volumes follow the now familiar pattern of a series of contributions by individual authors under a single editorship. It is the expressed

purpose of the authors primarily to present in critical fashion the principles rather than the particulars of individual topics, and to elaborate only in proportion as the lack of monograph or other specialist literature may require. For example, alkaloids receive only incidental mention and, following systematic treatment of simple butenolides, cardiac glycosides are dismissed with a general reference in the appropriate subsection which deals with naturally-occurring derivatives of furan. This policy should commend itself to the reader's purse, although it may not always serve his convenience and will evoke a critical attitude to the use which is made of the space gained.

Volume 1 comprises uncondensed heterocyclic compounds containing one hetero-atom, namely, oxygen, sulphur or nitrogen, in rings of three to six members. This accommodates (Chapters 1–3) discussion of ethylene (and trimethylene) oxides, of ethylenimines and of azete derivatives (azetidine = trimethylenimine) including β-lactams. Furans, thiophenes and pyrroles are assigned separate Chapters (4–6), which are followed (Chapters 7–9) by one on pyran/thiapyran types and by two on pyridines and their hydrogenated derivatives. Tribute must be paid to the authors no less for their clarity of style than for the wealth of information presented. In a composite work of this sort and in a field of this size, expedients of arrangement and occasional rather arbitrary selection of material are inevitable. Biotin, for example, a condensed bicyclic compound, takes up fully a quarter of the chapter on thiophene, whereas this chapter lacks the factual discussion of resonance to make it consistent with analogous sections. Again, the subsection which deals with reduced pyrroles is so severely restricted as to exclude even the mention of proline. Such criticisms, however, are overwhelmed by the merits of the book, which by its modern outlook, its range and readability will command the admiration as well as the interest of both the general and specialist reader.

Volume 2 deals with polycyclic compounds containing one oxygen or sulphur hetero-atom in a five- or six-membered ring. The first five chapters are devoted to the benzo-derivatives of furan, including phthalan and phthalide types, and of thiophene. Thereafter eight Chapters (6–13), all contributed by S. Wawzonek, present the various classes of compound derived from the benzopyran and dibenzopyran systems, the volume being completed by a Chapter (14) on thiochroman derivatives. A consistently high standard of treatment and presentation is maintained throughout, and the assemblage of much material that is otherwise widely dispersed will be particularly welcome. Attention is mainly directed to the fundamental chemistry of the heterocyclic systems, and where naturally occurring compounds are too numerous for detailed consideration, they are briefly described in tables (with references). It is regrettable, however, that these tables and their contents are not mentioned in the index.

Both volumes are well produced. The subject-matter is copiously illustrated with clear-cut, readily intelligible formulæ and is extensively documented by means of footnote references—many of them to recent work—providing a service of inestimable value. As judged by the present volumes, the series is conceived on a generous scale and when completed should go far to meet the requirements of all who have interests in the field of heterocyclic chemistry.

JAMES D. LOUDON

EMBRYONIC DEVELOPMENT IN THE SEED PLANTS

Plant Embryology

Embryogeny of the Spermatophyta. By Donald Alexander Johansen. (Plant Science Books, Vol. 24.) Pp. xviii+306. (Waltham, Mass.: Chronica Botanica Co.; London: Wm. Dawson and Sons, Ltd., 1950.) 6 dollars.

THE unique range of Prof. Johansen's information on the embryology of the seed plants gives him an obvious authority to write upon the subject. Moreover, there has always been a conspicuous gap in botanical literature in the English language for this book to fill. The author has presented a magnificent compendium of all the recorded observations on embryogeny, largely supplemented by his own researches. It is veritably a mine of information, from which little or nothing, positive or negative, seems to be missing. Indeed, the negative statements are often as valuable as the positive, provided that one bears in mind that such remarks as "nothing is known about so and so" refer only to the author's somewhat strict point of view.

The book is not, however, one which can be recommended for general student reading. It is as a work of reference that it makes its greatest appeal. One should except from this statement the introduction and Chapter 16, "Anthophyta; General Considerations", which deal with the history and scope of plant embryology.

The author has devised a system of algebraic notation for summarizing the sequence of cell divisions during embryonic development, which should be valuable to the research worker as a shorthand method of recording observations. It certainly gives greater precision to descriptions, but if applied too strictly to such plastic material it may prove deceptive. It is certainly difficult to follow without intimate acquaintance.

The first oospore division is taken as the starting-point for description, and therefore fertilization and embryo-sac structure are scarcely mentioned. Although this limitation is in accordance with plan and may have been inevitable in view of the amount of detail presented, it does detract from a rounded presentation of embryology such as is to be found, for example, in Maheshwari's recent book.

On p. 93, the author's plan of embryology is explained under three headings: (1) embryonic morphology, the static aspect or embryography; (2) embryonic physiology; (3) embryogeny, the kinematic aspect, with four sub-divisions—(*a*) embryogenesis, (*b*) embryotectonics, the architecture of the embryo, (*c*) embryogenergy, the destination or functions of the embryo, (*d*) embryonomy, the laws of embryonic development. The book is destined to expound only the last of these four.

The Gymnospermæ occupy eighty pages, while two hundred pages are given to the Angiospermæ, under the name Anthophyta. The families and genera included are compared and related by means of a system of types and variations. The word 'type' has been inherited from Schnarf, and his six type designations, named after exemplar families, have been retained, with one exception. The separation of these types is based primarily on the behaviour of the terminal cell of the two-celled embryo and secondarily on whether the basal cell does or does not contribute to the embryo proper. The classification

is simple, clear, and expresses differences of a fundamental nature.

Each type is sub-divided into a number of 'variations'. This term is unfortunate, for it has no genetical implication and is a purely formal expression of certain morphological concordances in embryonomy, grouping together plants which are otherwise unconnected. The term 'sub-type', though cumbrous, would have been better. Better still would have been the word 'pattern', which Johansen himself uses only in the Orchidaceæ, a family more or less *sui generis* embryologically. Terminology is, however, always a bone of contention between British and American authors and need not be further pursued here.

It is a reviewer's privilege to differ from his author; but he must not be ungenerous to his merits. This work has been so carefully compiled and it is so fully documented that it gives a very favourable impression of accuracy and reliability. The book is really a most notable addition to botanical literature, and it will retain its place and value for a long time.

R. C. McLean

HANDBOOK OF ENSILAGE

Silage

By Dr. Stephen J. Watson and Dr. A. M. Smith. (Agricultural and Horticultural Series.) Pp. 143+17 plates. (London: Crosby Lockwood and Son, Ltd., 1951.) 10*s*. 6*d*. net.

THE making of silage is no new thing in British farming. In the last quarter of the nineteenth century it had a considerable vogue, but the science of the process had not been worked out, mistakes were made and results were often disappointing; a few farmers persisted, but, speaking generally, silage-making languished for fifty years. During the Second World War great efforts were made to extend the practice, but these efforts had very little success. By this time much more was known of the changes which occur in green material when it is ensiled, but many farmers had not that knowledge and failed to appreciate that ensilage is merely a method of conservation and that it can never produce good feed from poor herbage. After the War farmers came to realize that the days of cheap imported feeding-stuffs were definitely ended, and not likely to return, and so they were ready to give ensilage a further trial and assign to it its proper important place in their farming systems.

Arable silage plays a part, but it is the ensiling of grass and clover which has been extended so much recently. Grassland provides by far the cheapest food for livestock, but the seasonality of its growth requires that the surplus of spring and early summer should be conserved for winter feeding The established method is haymaking; but this is dependent on the weather, and even in good conditions it entails considerable waste of nutrients. Artificial drying is increasing, but it is very expensive both for equipment and in operation. Ensiling grassland herbage costs less than half that of drying it artificially and, on the other hand, entails little more than half the nutrient loss that accompanies haymaking. It is the appreciation of these simple facts which has led to the remarkable increase in ensilage witnessed in the past two or three years, and it really does seem that the practice is now at last established in British farming.

There is no simple rule of thumb for making good silage. Details of the process must vary according to the material to be ensiled, the silo and equipment available, and to the weather at the time. For success, some knowledge of the principles involved is essential, and hitherto there has been no simple and complete handbook on the subject. Prof. S. J. Watson and Dr. A. M. Smith have met this need and met it very adequately. Their book is written in the plainest language; it deals sufficiently with the underlying principles and exhaustively with the details of the various methods. The book will be welcomed by farmers and by students, and it should do much to consolidate the position which silage-making has newly won.

H. G. Sanders

STUDIES IN ECONOMETRICS

Statistical Inference in Dynamic Economic Models

By Cowles Commission Research Staff Members and Guests. Edited by Tjalling C. Koopmans. (Cowles Commission Monographs, No. 10.) Pp. xv+438. (New York: John Wiley and Sons, Inc.; London: Chapman and Hall, Ltd., 1950.) 48*s*. net.

THIS book, to which eleven authors have contributed, represents the work which has been done by the members of the Cowles Commission and their collaborators during the past few years on the exploration of economic relationships by statistical methods. It is one of the most important works on econometrics which has yet appeared, and, although it is not easy to read, particularly for the economist with no special mathematical knowledge, it will be widely studied.

The basic problem of econometrics is to set up a model of the economic system and to formulate the equations which determine its behaviour. This raises questions which are distinct in degree and emphasis, though not perhaps in kind, from the parallel problems of writing down the equations of physics. The approach of the Cowles Commission is to formulate a set of linear equations in two kinds of variables: *endogenous*, namely, those inherent in the economics of the system; and *exogenous*, namely, those which impinge on the system from without. The effect of disturbances and the impossibility of experimentation are taken into account by allowing these variables to incorporate a random element. There are then four classes of problem for examination: to set up the system of equations; to determine whether they are soluble; to construct a system of estimation for the unknown parameters; and to compare the results in practical cases to see whether they are plausible enough to justify acceptance of the model.

The basic memoir in this volume is that by Koopmans, Rubin and Leipnik on "Measuring the Equation Systems of Dynamic Economics". The other articles develop various aspects of the subject in dealing with problems of identification, structural and predictive estimation and time-series. The papers, considered as a whole, are of a high standard; but the connectivity of the treatment suffers from the fact that they are by different authors, there is no clear set of definitions of some of the terms employed, and no practical examples are given. This is therefore a book for the specialist. The Commission promises a further volume of a more expository character in which emphasis will be placed on basic ideas and techniques rather than on mathematical proofs.

Colloques Internationaux du Centre National de la Recherche scientifique, 14

Méthodes de calcul dans des problèmes de mécanique, Marseille, 30 Mars–6 Avril 1948, Paris, 8–9 Avril 1948. Pp. 105. (Paris: Centre National de la Recherche scientifique, 1949.) 900 francs; 20*s*.

THE subject of these lectures is more restricted than might be anticipated from their title. The first part of the colloquium was held at Marseilles, where it was hoped to establish a mathematical laboratory, and opened with an appreciation of British methods by J. Valenski (Marseilles). The proceedings consisted chiefly of an exposition of Southwell's well-known 'relaxation method', delivered by D. N. de G. Allen, one of Sir Richard Southwell's colleagues. A later British method, the 'escalator method' of Morris and Head, was explained by T. Vogel (Marseilles); it has roughly the same relation to dynamics as Southwell's method has to statics.

In the second part of the colloquium, held at Paris, M. Picone (Rome) dealt with the analysis of periods, a problem of great importance in astronomy, meteorology, statistics and other branches of science. This problem has been studied by H. and Mme. Labrouste (Paris) and by F. Vercelli (Italy). An account of the 'Logobax' machine which can perform the calculations needed in the Labrouste method was given by L. Couffignal (Paris). J. M. Burgers (Delft) discussed some problems of turbulent motions in fluids. L. Malavard (Paris) dealt with calculations by the aid of electric circuits, and their application to hydro- and aero-dynamics. A. van Vijngaarden (Amsterdam) and F. H. van den Dungen (Brussels) dealt with other methods applicable to hydrodynamics. Finally, L. Couffignal discussed in general terms the role of numerical calculation in scientific and technical research.

The organizers deliberately excluded consideration of mechanical and electronic calculating machines on the ground that a subject so vast would require a whole colloquium to itself. H. T. H. P.

Fluorine Chemistry

Edited by Dr. J. H. Simons. Vol. 1. Pp. xvii+615. (New York: Academic Press, Inc., 1950.) 12 dollars.

"FLUORINE", it has been stated by Prof. N. V. Sidgwick, "is in some ways the most interesting of the halogens." Yet the importance, both real and potential, of the element and its compounds has become apparent only in recent years. Extensive and vigorous researches have occurred since 1940, and have rendered obsolete and inadequate the two classical works on fluorine by Henri Moissan and Otto Ruff. For this and other reasons, the publication of a new and thorough treatise on fluorine chemistry is timely and welcome.

The first volume of this work is written by a group of fifteen authors, each dealing with a branch of the subject in which he has personal experience. The inorganic chemistry of fluorine is described in detail by H. J. Emeléus, A. B. Burg, H. S. Booth, J. T. Pinkston, jun., D. R. Martin and J. H. Simons—special attention being given to the halogen fluorides, boron trifluoride and hydrogen fluoride. The preparation of elementary fluorine—now no longer a laboratory curiosity, but an item of industrial production—is dealt with by G. H. Cady. Some theoretical aspects of fluorine chemistry are examined by George Glockler in one of the more speculative chapters.

Perhaps the most fascinating sections of the book are those by L. A. Bigelow, J. H. Simons, T. J. Brice, W. H. Pearlson, and J. D. Park, in which the fluorocarbons and their derivatives are described. This field is potentially one of the most enormous in chemistry, for the fluorocarbons are not only more stable than hydrocarbons, but form large numbers of derivatives.

Altogether, this is an excellent production, authoritatively written. H. A. SKINNER

Practical Dairy Bacteriology

By Prof. Paul R. Elliker. (McGraw-Hill Publications in the Agricultural Sciences.) Pp. ix+391. (New York and London: McGraw-Hill Book Co., Inc., 1949.) 34*s*.

THIS book, as the preface proclaims, is intended for the elementary student. Bacteriological descriptions of the organisms encountered in the production and processing of milk and the manufacture of dairy products are not seriously attempted. The treatment is mainly technological, and in this respect the book can safely be recommended to the operative and to the student in this field. References abound in all the chapters, but these are, with few exceptions, to American publications.

Flamingo City

By G. K. Yeates. Pp. 210+38 plates. (London: Country Life, Ltd.; New York: Charles Scribner's Sons, 1950.) 25*s*. net.

THIS can fairly be said to be an outstanding bird book. Mr. G. K. Yeates is already well known as a bird photographer; but this work shows that he can also write very well indeed. His chapters on the flamingo colony of the Carmargue do not exhaust the interest of the book, although they are its highlights. Mr. Yeates writes on the other birds of Provence, and we have an insight of the beauty of their colouring and the attractive feature of their lives in that sun-drenched land which has only two disadvantages—the mistral and the mosquito. The strange habits of the pratincole and the domestic happiness of the brilliantly plumaged bee-eater are told clearly and arrestingly. The mind of the reader is indeed held throughout the chapters of the book, so that the author's disappointments and his most exciting moments are shared.

In a book where the illustrations are of so high a standard, it is difficult to single out any specially for praise; but the coloured photograph of the flamingo group on p. 158 is truly magnificent. Scarcely less beautiful are the illustrations (p. 62) of the pratincole on the nest, and of the two bee-eaters on the sand (p. 90). The author has interesting things to tell us of the unusual nesting habits of the flamingo. It does not necessarily nest each year, unless the water-level is to its liking. The food of this large and strikingly beautiful bird is not known with certainty. Gallet, who has studied the flamingo more closely than any other living observer, has advanced the novel and bold theory that the bird feeds on putrid mud. This would account for the nesting of the flamingo in a terrain so saturated with salt as to support no apparent life.

The publishers have done their part of the work admirably, and have produced a book which must add distinction to the library of the owner fortunate enough to possess it. SETON GORDON

A CENTURY OF PHYSICS*

PHYSICS FROM 1850 to 1900

By Prof. E. N. da C. ANDRADE, F.R.S.

ONE characteristic of this great period of physics is the complete dominance of Newtonian mechanics; this dominance does not, it is true, distinguish it from the previous fifty years, but does offer a contrast with the period with which Prof. Born is to deal. What, perhaps, characterizes the period more closely is the way in which Newtonian mechanics was applied outside the field of astronomy, and the mechanics of visible masses in general, to which it had been more or less restricted before 1850. In all the various ether models which were a feature of nineteenth-century physics Newtonian mechanics found expression, as the Newtonian system did, indeed, in all the early atom models. But perhaps its most striking direct expression was in the kinetic theory of gases, the creation and development of which to a high degree of precision is almost entirely the affair of this period.

The period is further characterized by the prominence which electrical forces came to attain and the great generalization of the relation between electric and magnetic forces which, at the hands of Maxwell and Hertz, led to the formulation and triumph of the electromagnetic theory of light. The first establishment of the wave theory of light, by Young and Fresnel, in the early years of the past century, necessarily involved the conception of an ether of space: whereas the light particles of Newton could travel through empty space, the propagation of waves seemed to demand a medium, something in which the waves could travel (I must add, in parenthesis, that Newton's particles had certain wave aspects, so that his theory, like that which prevails to-day, demanded for light mixed wave and particle properties). The ether of space, however, if it is to express the experimental facts, must have very peculiar properties, and the history of physics from 1850 to 1900 is characterized by the extraordinary skill, imagination, mathematical ability and physical credulity used in the construction of ethers. Let us glance at some of the steps by which the ether attained its command over much of the physics of the period in question.

The work of Oersted in 1820 first showed that an electric current produced a force that acted upon a magnetic pole—a magnetic force—and Ampère worked out a beautiful mathematical theory of this effect and of the mutual action of steady currents. Faraday developed the laws of electromagnetic induction in the 1830's and put forward a general theory of electric and magnetic action in terms of lines of force. One great contrast between Faraday's views of what we now call the electromagnetic field (the term 'magnetic field' seems to have been first used by Faraday in 1845, just before the period opens) and the views which were then current is the emphasis which Faraday placed on the part played by the medium. In 1851, when the period opens, he published a paper "On lines of Magnetic Force: their definite character: and their distribution within a Magnet and through space" which gives the results of his mature reflexion and develops the conception of cutting lines of magnetic force in connexion with induced currents. This was against the feeling of his times, especially the feeling of the mathematicians. Airy, the Astronomer Royal, for example, said of a slightly later paper in which Faraday had further developed his views on lines of force, "but I declare that I can hardly imagine anyone who practically and numerically knows this agreement [that between the inverse square law of attraction and observation] to hesitate an instant in the choice between this simple and precise action, on the one hand, and anything so vague and varying as lines of force, on the other hand". That was written in 1855. In the same year Clerk Maxwell wrote his first paper on electricity, an elaborate treatise which occupies seventy-five pages in his collected works, entitled "Faraday's Lines of Force". Here he showed that the conception of lines of force could be made to yield precise mathematical results when suitably treated. He wrote: "By a careful study of the laws of elastic solids and of the motion of viscous fluids I hope to discover a method of forming a mechanical conception of the electrotonic state adapted to general reasoning". This sentence expresses as a particular case the general ambition of the man of science of the period—to find the mechanical characteristics which would endow an ether with the necessary properties. This is probably the only occasion on which, in this short paper, I shall have an opportunity of referring to another characteristic feature of the period, the great development of the mathematical theory of the perfect elastic solid and of the perfect fluid, represented by the work described in the great text-books of Love and Lamb. As for real fluids, Newtonian viscosity was just tolerated; but real solids, which did not obey Hooke's law, or, still worse, which showed plastic deformation, were not mentioned in decent scientific society.

A few years after this first paper Clerk Maxwell developed his project and put forward the electromagnetic theory of light. He used illustrations from the motion of a medium governed by Newtonian laws; for example, he conceived magnetic lines of force as vortex filaments. He introduced the conception of a displacement current: when one plate of a parallel-plate condenser was being charged, something between the plates was being elastically displaced, so that the current was continuous. From his equations he deduced that electromagnetic waves must exist, estimated their velocity, found that it agreed with that of light and concluded, in his own words, "We can scarcely avoid the inference that light consists of the transverse undulations of the same medium which is the cause of electric and magnetic phenomena". This is one of the great unifying discoveries of physics. I can only refer to Maxwell's classical "Treatise on Electricity and Magnetism", which is, in the main, a precise quantitative expression of Faraday's general views. Stresses in the medium play a prominent part. Maxwell deduced the pressure of light, which was later investigated experimentally by Lebedew, Nichols and Hull, and Poynting, and

* Substance of three papers read on August 13 before Section A (Mathematics and Physics) of the British Association meeting at Edinburgh.

has proved of such importance in connexion with astronomical problems and thermodynamic theory. Poynting's name reminds us of his great theoretical discovery, the Poynting vector.

Following Maxwell, Hertz, having first deduced Maxwell's equations on a different basis, proceeded to realize electromagnetic waves experimentally, by means of the oscillating spark discharge. He invented the resonant circuit. Hertz, followed by FitzGerald and Trouton, showed that all the properties of light, reflexion, refraction, polarization, could be demonstrated with the electromagnetic waves from the spark discharge. This irrefutably established the electromagnetic theory of light. I need scarcely refer to the extraordinary developments that have flowed from Hertz's work.

Meanwhile, the search for a satisfactory mechanical ether continued to occupy many of the great names of theoretical physics. Even if the chief result of all this brilliant and sustained effort was to show that such an ether was impossible, I cannot pass it over without mention. Fantastic values for the elasticity and density of the elastic medium which was postulated by many were worked out. Vortices, mixtures of vortices and irrotational elements to make what Kelvin called a vortex-sponge, an aggregate of vortex rings and other kinds of vortex elements were made to bear cumbrous loads of mathematics which now awaken the kind of wonder evoked by the Pyramids—prodigious labour to enclose a small chamber empty of anything that has significance for us. As late as 1903 Osborne Reynolds published a book of 250 pages on an ether consisting of uniform spherical grains beginning, "By this research it is shown that there is one, and only one, conceivable purely mechanical system capable of accounting for all the physical evidence, as we know it, in the Universe", which reminds one of Newton's "It remains, that from the same principles, I now demonstrate the frame of the System of the World", the only difference being that Osborne Reynolds's statement is the inscription on a tombstone of the dead and forgotten and Newton's is on a triumphal arch. It was with reference to Osborne Reynolds's ether that FitzGerald said "Hard particles are an abomination". It is only of recent times that we have come to accept that the ether is no more than a subject for the verb "to undulate" and that Clerk Maxwell's theory is simply Clerk Maxwell's equations.

We cannot leave this cemetery of dead ethers, however, without recounting one consequence of inestimable importance which the search for a satisfactory ether produced. If there were a stationary luminiferous ether, the motion of the earth through it would appear to have as a consequence that the velocity of light in the direction of motion, as measured by an observer on the earth, should be slightly different from that at right angles to this direction. Albert Michelson, with the collaboration of Morley, undertook the task of trying to establish this difference of velocity by an apparatus based on the interference of two beams of light which had travelled at right angles to one another. The sensitiveness of the method, which was an optical triumph, was amply sufficient to establish the motion through the ether, if the reasoning on which it was anticipated was correct. Everybody knows that the result was negative. This result, supported by the failure of other attempts to detect a motion through the ether, was the only begetter of the first theory of relativity, which graced the early years of the succeeding period.

I should here just refer to the experiments on the velocity of light. They begin in 1849 with Fizeau's toothed-wheel method, much improved twenty-five years later by Cornu. Foucault's rotating mirror method enabled much shorter light paths to be used. Michelson effected great improvements and obtained in 1882 a value 299,853 km./sec., which compares well with the value 299,792 recently obtained by Essen—sixty-five years later—a difference of 1 part in 5,000.

The theory of heat as we know it is, with the exception of Carnot's fundamental work (1824), practically the creation of the period 1850–1900. Perhaps I should also mention as a forerunner Fourier, of the importance of whose work there is no question, but his "Théorie Analytique de la Chaleur" (1822) is a mathematical development, not depending on the nature of the heat which is assumed to pass at a rate proportional to the temperature gradient. The first prime advance to which I will refer is the establishment of heat as a form of energy. Julius Robert Mayer's first announcement, in which he calculated a value of the mechanical equivalent of heat, goes back to 1842 and 1845, just before our period, but it received no recognition, rather the opposite. The date 1850 is remarkable as that on which Mayer, probably as a result of persecution, partly real, partly imagined, jumped out of a window and received severe injuries. It was owing to Tyndall that his work first received due recognition in 1862. Joule it was, however, whose work really established in a convincing manner the equivalence of work and heat and gave an accurate value of the mechanical equivalent. This work extended over the period 1843–78. The experiments on the heat generated by stirring water with a measured expenditure of work are described in all the text-books: what is often forgotten is that Joule also measured the heat generated by the current from a dynamo driven by a measured power; by the compression of a gas; and by the forcing of water through fine tubes. The value he obtained in his best experiments on stirring water (1878) was $4{\cdot}184 \times 10^7$ ergs as equal to a standard (15°) calorie: the accepted value to-day is $4{\cdot}181 \times 10^7$ ergs, a difference of less than 1 part in 1,000. Reference should be made to Rowland's extremely careful determination, which gave the value 4·187.

That the various determinations, made by widely different methods, gave the same value of the mechanical equivalent of heat means the experimental establishment of the first law of thermodynamics, which was explicitly enunciated by Clausius in 1850. At the same time Clausius formulated the second law of thermodynamics, which was independently put forward by William Thomson (Lord Kelvin) in a slightly different form. Clausius and Kelvin may rank together as the founders of modern thermodynamics.

The next fifty years saw the rapid development of the two methods of treating thermal phenomena, one the method of thermodynamics, which makes no assumptions as to the nature of heat but deals with its transformation into work according to general laws, and the other the method of statistical mechanics, which assumes the atomic structure of matter and calculates heat effects and transmutations as statistical averages of atomic events. Clausius introduced the term and conception entropy, to express the availability of energy in a thermodynamic system. The two laws of thermodynamics may be expressed as (i) the energy of the universe is constant;

(ii) the entropy of the universe tends to a maximum. Classical thermodynamics rests on the principles of energy and entropy, both defined without reference to the structure of matter. The introduction of the conception of entropy, which is a one-valued function of the state of a system, is one of the great achievements of our period.

It is impossible here to trace the great developments of thermodynamics that, at the hands of Maxwell, Kelvin, Kirchhoff, Helmholtz, van t'Hoff, Willard Gibbs and others led to results of such importance for physics, physical chemistry and engineering. The foundation of the absolute scale of temperature is one great landmark. The application, initiated by van t'Hoff, of thermodynamics to the study of chemical equilibrium—we may mention, as indicative, van t'Hoff's isochore—founded a new branch of science, which has had immense consequences for industrial chemistry. Willard Gibbs, whose work lies entirely in this period, deserves, perhaps, a special word. When, in 1901, the Copley Medal of the Royal Society was awarded to him, the citation said that he was "the first to apply the second law of thermodynamics to the exhaustive discussion of the relation between chemical, electrical and thermal energy and capacity for external work", which summarizes the chief advances of thermodynamics in the last decades of the century. The phase-rule was one of his achievements. I may, perhaps, quote Einstein's words referring to certain papers which he wrote in 1902 and 1903: "Wenn mir das Gibbsche Buch damals bekannt gewesen wäre, hätte ich jene Arbeiten überhaupt nicht publiziert, sondern mich auf die Behandlung einiger weniger Punkte beschränkt".

The application of thermodynamics to radiation was another step of first importance. It was as long ago as 1884 that Boltzmann showed that, by considerations involving the pressure of radiation, Stefan's laws of radiation—that the total energy of radiation of a black body is proportional to the fourth power of the absolute temperature—could be proved thermodynamically. The introduction of the conception of a black body, or full radiator, by Kirchhoff reminds us of the great services of this man. And a close link between this period and that which follows is furnished by a paper written in 1900 by W. Wien, in which he deduced a general formula for the distribution of energy in the spectrum of a black body. What the development of this problem led to at the hands of Planck is well known: it is fundamental for modern physics.

As an experimental physicist I cannot pass over the brilliant work that established experimentally the distribution of energy in the spectrum of a black body, for this, after all, is what guided theory. The work of Lummer and Pringsheim came right at the end of the century, while that of Rubens and Kurlbaum on the infra-red falls precisely in 1900.

So much—or rather so little—for thermodynamics. Let us look at the other method of approaching the problem, the molecular and statistical. The development of the kinetic theory of gases is a capital feature of the period, and led directly to the more comprehensive statistical mechanics. The only work that comes before 1850 is that of Waterston, which had no influence, as it was not published until 1892! It was Krönig (roughly) closely followed by Clausius (more precisely) who early in our period showed that the simple laws of the pressure of gases could be considered as a consequence of molecular bombardment of the walls of the container. Clausius soon introduced the important conception of mean free path. It was Maxwell and Boltzmann who were responsible for some of the chief mathematical developments of the theory. Maxwell established the formula for the distribution of energy among the molecules: the molecules, moving and colliding at random, have a wide range of velocity—theoretically from zero to infinite—and Maxwell's distribution law tells us what fraction of them have velocities within a given small range, when the gas is in statistical equilibrium. It was, perhaps, Boltzmann who by his method of treatment founded statistical mechanics, of which my colleague will, no doubt, have something to say. In particular, he showed that entropy could be expressed as the logarithm of a probability, and so tied thermodynamics and statistical methods together. Before leaving the subject, I may perhaps refer to Maxwell's astonishing deduction from the kinetic theory that the viscosity of gases is independent of the pressure, which he proved experimentally. This prediction was a triumph for the kinetic theory.

A great advance was made when the behaviour of real gases, as distinct from ideal gases obeying Boyle's and Charles's laws, was investigated both experimentally and theoretically. Regnault and Natterer had established just before our period opens that certain gases deviated from these simple laws. It was in 1869 that Andrews published his classical work on carbon dioxide, which established the existence of a critical point and made it clear that gases can be liquefied only if below a certain temperature peculiar to each. A few years later J. D. van der Waals developed his theory of real gases in a paper entitled significantly "The Continuity of the Liquid and Gaseous States in Matter". The theory is epitomized in his famous equation, which he was able to compare with Andrews's results. From it he deduced the theory of corresponding states, namely, that if we express the volume, pressure and temperature of a gas in terms of the critical volume, pressure and temperature of that gas, then one equation without arbitrary or experimental constants expresses the behaviour of all gases. It is not, of course, exact, but approximates surprisingly well to reality. Van der Waals's equation was followed, of course, by many other equations of state, of which the best is probably that of Dieterici.

The work of Andrews and van der Waals was, naturally, of great importance for the problem of the liquefaction of gases: it explained the failure of many attempts to produce liquid gases at a temperature above the critical. In 1883 Wroblewski and Olzewski liquefied oxygen, and soon after Olzewski liquefied oxygen, nitrogen, air and carbon monoxide in large quantities, and saw liquid hydrogen in an evanescent way. Just before the end of the century Dewar liquefied hydrogen in appreciable quantities, making use of his beautiful invention, the vacuum flask. Helium stood out until after the turn of the century; but the Leyden Cryogenic Laboratory, where Kamerlingh Onnes achieved this triumph, was founded by him in 1885. The liquefaction of gases is a good example of how experiment and theory alternate, each experimental stage producing a theoretical advance and vice versa. What modern theory owes to the experimental production of low temperatures is well known; I need only mention specific heats and superconductivity.

Round about 1890 the ether seemed to be the chief outstanding problem of physics: otherwise everything was in very fair order and nice and quiet. The

index of *Nature* about that time collects the entries that are taken to refer to the subjects physics and chemistry. Taking two consecutive volumes I find in the index 29 lines referring to physics and about 240 to chemistry, and turning the pages of these volumes seems to show the same peaceful atmosphere in our subject. However, a burst of new discoveries—I do not say entirely unheralded—was about to break upon the scientific world. In his presidential address to Section A at the Edinburgh meeting of the British Association in 1892 Schuster remarked: "The most puzzling fact, however, connected with the discharge of electricity through gases"—a subject that interested him—"consists in the absence of symmetry at the positive and negative poles". That was the key to one great advance about to be made, the discovery of the electron as the result of the study of the low-pressure discharge.

As early as 1874 Johnstone Stoney, from the facts of electrolysis, had concluded that electricity was atomic in nature and had later (1891) suggested the name electron for the unit. The history of the actual establishment of the electron is a complicated matter. Perrin established the negative nature of the cathode stream. J. J. Thomson in 1897 showed that the ratio mass to charge of the particles, if particles they were, was about a thousandth of that for the hydrogen ion in electrolysis, but did not conclude as to the magnitude of the charge. Lenard, from his experiments on the passage of cathode rays through metals, concluded that the particles must be much smaller than atoms. Townsend in 1897 and 1898 attempted a direct determination of the electronic charge and obtained a value about two-thirds of the true figure. H. A. Lorentz showed that the effect discovered by Zeeman in 1897 could be explained on the assumption that the light emission was due to a particle with the properties of the electron. We can say that by the end of the century the existence of the electron was generally accepted, although its charge had not been very well established.

But these last years of the century were full of excitement, full of discoveries which came to full fruition in Born's period. In 1895 Röntgen discovered X-rays, as he named them: further, he himself established with astonishing rapidity their chief properties. They caused a popular sensation by enabling the bones of the human hand to be photographed through the flesh; but in the physical laboratories the ionization which they produced was the chief study. The photoelectric effect had been established by Hallwachs in 1888: in 1899 Lenard proved that the negative electricity released from metals by light consisted of electrons, as independently demonstrated by J. J. Thomson shortly after. In 1896 Henri Becquerel discovered that uranium salts emitted rays which affected a photographic plate, and that it was the uranium atom itself that was responsible for the radiation. This was the beginning of the study of radioactivity. Rutherford's first paper on the subject, which was an impressive piece of work, appeared in January 1899 under the title "Uranium Radiation and the Electrical Conduction produced by it". Meanwhile, in France, Marie Curie had discovered polonium and in 1898 Pierre and Marie Curie made the sensational discovery of radium. In the same year, Schmidt and Marie Curie independently established the radioactivity of thorium. The year after, Debierne produced actinium, and the first emanation, that of thorium, was discovered in the last year of the century. The study of radioactivity was, then, well launched in the early years. Finally, perhaps, if we are speaking of the birth of modern physics, reference ought to be made to the discovery of the positive canal rays, the father of isotopes, by Goldstein in 1886. Corpuscular physics, with its vast possibilities, was born in those last five years of my period.

I realize only too keenly how imperfectly I have surveyed the field. Every physicist here will accuse me, and accuse me with justice, of having made no reference to matters and men of capital importance. I have said, for example, nothing of the great work of Lord Rayleigh on sound—nominally on sound, but the mathematical methods and results exposed in his great book have far wider import, and have, for example, recently been used for problems connected with wave guides. I have said nothing of the discovery of the rare gases, I have said nothing of a vast and significant body of experimental research on heat and light, particularly on spectroscopy: nothing of the determination of the gravitational constant. I can only plead that forty minutes is a short time in which to deal with so much of really prime importance.

PHYSICS IN THE LAST FIFTY YEARS

By Prof. MAX BORN, F.R.S.

THE following review is based on personal recollections and cannot claim historical accuracy and completeness. I shall tell you what has impressed me most, since I attended, in 1901, my first lecture at the University of Breslau, my home city. We were taught what is called to-day classical physics, which was at that time believed to be a satisfactory and almost complete description of the inorganic world. But even Maxwell's theory of the electromagnetic field was, about 1900, not a part of the ordinary syllabus of a provincial German university, and I remember well the impression of bewilderment, admiration and hope which we received from the first lecture on this subject given to us by the then young and progressive lecturer Clemens Schaefer (still active at Cologne).

The first great event of a revolutionary character happened in 1905 with Einstein's theory of relativity. I was at that time in Göttingen and well acquainted with the difficulties and puzzles encountered in the study of electromagnetic and optical phenomena in moving bodies, which we thoroughly discussed in a seminar held by Hilbert and Minkowski. We studied the recent papers by Lorentz and Poincaré, we discussed the contraction hypothesis brought forward by Lorentz and FitzGerald and we knew the transformations now known under Lorentz's name. Minkowski was already working on his four-dimensional representation of space and time, published in 1907, which became later the standard method in fundamental physics. Yet Einstein's simple consideration by which he disclosed the epistemological root of the problem (the impossibility of defining absolute simultaneity of distant events because of the finite velocity of light signals) made an enormous impression, and I think it right that the principle of relativity is connected with his name, though Lorentz and Poincaré should not be forgotten.

Although relativity can rightly be regarded as the culmination of nineteenth-century physics, it is also

the mainspring of modern physics because it rejected traditional metaphysical axioms, Newton's assumption about the nature of space and time, and affirmed the right of the man of science to construct his ideas, including philosophical concepts, according to the empirical situation. Thus a new era of physical science began by an act of liberation similar to that which broke the authority of Plato and Aristotle in the time of the Renaissance.

That result of relativity which later proved to be the most important, namely, the equivalence of mass and energy as expressed by the formula $E = mc^2$, was at that time considered to be of great theoretical, but scarcely of any practical, interest.

In 1913 Einstein's first attempt on general relativity became known; it was perfected two years later. It is the first step not only beyond Newtonian *meta*-physics, but also beyond Newtonian physics. It is based on an elementary but so far unexplained fact—that all bodies fall with the same acceleration. To this day it is this empirical foundation which I regard as the corner-stone of the enormous mathematical structure erected on it. The logical way which led from this fact to the field equations of gravitation seems to me more convincing than even the confirmation of the astronomical predictions of the theory, as the precession of the perihelion of Mercury, the deflexion of light by the sun and the gravitational shift of spectral lines.

Einstein's theory led to a revival of cosmology and cosmogony on an unprecedented scale. I am not competent to judge whether it was the theory which stimulated the astronomers to build bigger and more powerful instruments, or whether the results obtained with these, like Hubble's discovery of the expanding universe, stimulated the theoreticians to still loftier speculations about the universe. The result, however, is undoubtedly that our astronomical horizon to-day, in 1951, is vastly wider, our ideas about the creation vastly grander than they were at the beginning of the period. We can estimate the actual age of the world (some thousand millions of years), its present size (determined by the receding nebulæ reaching the velocity of light) and the total number of nebulæ, stars and atoms, and we have good reasons for assuming that the laws of physics are the same throughout this vast expanse. The names of Friedmann, Lemaître, Eddington and Robertson must here be mentioned.

But after this boast let me conclude this section on a note of modesty. The fundamental problem of connecting gravitation with other physical forces, to explain the strange value of the gravitational constant, is still unsolved in spite of Eddington's obstinate, ingenious attempts. The most promising idea seems to me that of Dirac, developed by Jordan, that the gravitational constant is not a constant at all, but a scalar field quantity, which like the other ten, the components of the metric tensor, undergoes a secular change and has acquired its present value in the course of time elapsed since the creation of the universe.

Before speaking about the most characteristic features of modern physics, atomistics and the quantum concept, I have to dwell for a short time upon classical physics which, of course, has not suddenly ceased to exist, but continues and flourishes to such a degree that I should venture to say by far the greatest part of the time and effort of physicists is still devoted to problems of this kind, even of those, frequently found in the United States, who believe that nuclear research is the only decent pursuit deserving the name of physics.

In fact, the progress and success since 1900 in ordinary mechanics, elasticity, acoustics, hydro- and aero-dynamics, thermodynamics, electrodynamics and optics is spectacular enough. You have only to remember that in 1900 the internal combustion engine was in its infancy, motor-cars often brought in by horses and the aeroplane a fantastic dream. It would be impossible to attempt even the crudest sketch of these and other technical developments due to physics. Let me only mention a few characteristic points.

The first is the adoption of a more realistic attitude. In the nineteenth century the mechanics of solids and fluids were beautiful mathematical theories well suited for providing examination papers. To-day, they tackle actual problems of daily life and technology, for example, in hydrodynamics, boundary layers, heat transfer, forces on moving rigid bodies like the wings of aeroplanes, the stability of these, even for supersonic velocities. Among the pioneers whom I personally knew are G. I. Taylor, Prandtl, Kármán. In elasticity we have a similar development; the narrow field of problems accessible to analytical solutions has been enormously extended by numerical methods (Southwell's relaxation method), and the results are checked by photoelastic observations on transparent models.

This trend has been strongly assisted by the invention of mechanical and electrical computing machines. The speed and power of the modern instruments based on electronic valves has stirred the imagination of the world and given rise to a new science, cybernetics, the advocates of which expect a revolution of human civilization from these artificial brains—a belief which I do not share.

Acoustics, the branch of elasticity dealing with the propagation of waves, was confronted by numerous problems through the invention of the gramophone, the telephone and broadcasting. Here again the electronic valve was a powerful tool. Ultrasonic vibrations have been used for studying the elastic properties of crystals, for signalling and for time-keeping. The clock controlled by the oscillations of a piezo-quartz crystal seems to be more accurate and reliable than ordinary pendulum clocks.

Prof. Andrade has given an account of the origin and the development of thermodynamics which in 1900 was considered to be complete, with its two fundamental theorems (conservation of energy, increase of entropy). But this complacent conviction was wrong here as in many other cases.

In 1907 Nernst added a third theorem concerning the behaviour of substances at zero temperature. Of its numerous applications to physics and physical chemistry I can only mention the prediction of chemical equilibria and reactions, as exemplified by Haber's method of fixing nitrogen from the air (1914). The experimental approach to absolute zero made great strides. Keesom arrived in 1931 at 0·7° K. with the help of liquid helium. Giauque and MacDougall devised in 1933 a new method for cooling, using the demagnetization of paramagnetic salts. The absolute scale of temperature was extended below 1° K. by Kurti and Simon (1938) and others. Strange phenomena were discovered in this region, the supraconductivity of metals by Kamerlingh-Onnes in 1911, and the superfluidity of liquid helium by Keesom and Wolfke in 1927, Allen and Misener, Kapitza and others.

Even at higher temperatures new phenomena were found, for example, in the field of highly concentrated electrolytic solutions where the names of Bjerrum, G. N. Lewis, Debye and Hückel must be mentioned.

An approach to extreme conditions from another angle was made by Bridgman (since 1905), who systematically investigated the properties of matter under high pressure, reaching more than 100,000 atmospheres. His latest triumph is the observation of the breakdown of the electronic shells of alkali atoms under pressure.

Of great importance seem to me the recent investigations started by Onsager in 1930 and continued by Casimir, Prigogine, de Boer and de Groot, by which thermodynamics is generalized so as to apply to irreversible processes, by combining the classical laws of flow with one single result of statistical mechanics, the so-called principle of microscopic reversibility. The results seem to have a bearing on the understanding of the processes going on in living organisms.

The progress of electrodynamics is obvious to everybody in technical applications: improvements in the production of power and its transmission over long distances; telecommunication methods, such as telegraphy, telephony and wireless transmission. In 1900 electromagnetic waves were a laboratory experiment. Since Marconi's success in 1895 broadcasting has become a powerful factor in human affairs.

Electromagnetic waves comprise the whole of optics, but it would be quite impossible to give an account of the progress in all branches of optical research and practice. The improvements and refinements of all kinds of optical apparatus, of the experimental and theoretical investigation of diffraction, refraction, absorption and scattering are enormous. Let me mention only a few outstanding achievements in spectroscopy because of their bearing on atomic physics: the discovery of the Zeeman and Stark effect, the disentanglement of spectral series by Rydberg, Paschen, Runge, Ritz and others, the Raman effect, the extension of the spectrum towards the ultra-violet and infra-red, and finally the closing of the gap, still existing in 1900, between the longest light or heat waves and the shortest radio waves. The pressure of war helped to develop the method known as radar. In the laboratory it provided the magnetic resonance effect, used for the study of atoms, molecules, crystals (Cleeton and Williams, 1934; Griffith, 1948), and even for the determination of nuclear spin and quadrupole moments (Rabi, 1938). It has also enriched our knowledge of the world at large by the application to the ionosphere (Appleton and Barnett, Breit and Tuve, 1925) and to celestial bodies. Reflexions have been obtained from the moon (U.S. Signal Corps, 1948) and from meteors (Hey and Stewart, 1946), and waves coming from the Milky Way (Janski, 1931) have been observed. This new radio-astronomy will have a profound influence on cosmology.

We now come to atomistics. Although firmly established in the nineteenth century, there were still, in 1900, some distinguished physicists who did not believe in atoms. To-day, such people would be regarded as 'cranks', since the evidence for the atomistic structure of matter is overwhelming.

There are two different but closely interwoven problems to be answered by atomistics: (1) What is the nature of the atoms? (2) How can the behaviour of matter in bulk be accounted for in terms of the collective action of atoms?

Let us begin with the latter question as it has been answered for a special type of matter already in the nineteenth century; I mean the kinetic theory of gases and its extension to more general systems in statistical equilibrium through Gibbs's statistical mechanics. This was in 1900 a reasonable hypothesis. But Einstein's explanation of the Brownian movement in 1904 and Smoluchowski's consecutive work in 1906 provided direct physical evidence for the correctness of the kinetic theory and led Perrin in 1909 to a reliable value of the number of atoms in the gram-molecule.

The theory of compressed gases and condensation started by van der Waals in 1873 has been much improved and modernized by Ursell (1927), Mayer (1937) and others.

A statistical treatment of paramagnetism was given by Langevin in 1905, and extended to ferromagnetism by Weiss in 1907. This was the first example of a type of statistical problem dealing with so-called order–disorder phenomena, to which, for example, the properties of alloys belong. These methods are to-day of great practical importance.

The logical foundations of statistical mechanics were critically examined by Paul and Tatyana Ehrenfest (1911) and its mathematical methods vastly developed by Darwin and Fowler (1922).

While a satisfactory kinetic theory of liquids, in spite of great efforts, is still lacking even to this day, our knowledge of the solid state has been greatly increased. This work is closely connected with research on X-rays. The nature of X-rays was controversial until 1912. Selective absorption and polarization discovered by Barkla in 1909 indicated wave structure. A year later W. H. Bragg found evidence for corpuscular structure. In 1912 Pohl and Walter obtained diffraction at a slit from which Sommerfeld estimated the wave-length. The dispute was finally settled in favour of waves when Laue and his collaborators found, in 1912, diffraction of X-rays by crystals, demonstrating at the same time the atomistic nature of solids, the lattice structure of crystals, which had been hypothetically assumed for a long time.

In the hands of W. H. and W. L. Bragg this method opened a new science, atomistic crystallography, which abounds in ingenious experiments and mathematical considerations, as the systematic application of group theory initiated by Sohnke as early as 1879 and perfected by Schönflies and Fedorow in 1891.

Upon this empirical geometry of crystal lattices there has been erected a dynamical theory which actually started as one of the first applications of quantum theory with Einstein's work of 1907 on the specific heat of solids at low temperatures, and its refinements by Debye and by Kármán and myself in 1910, which, however, has also a large field of application in the classical domain, predicting relations between elastic, thermal and optical properties of crystals. While for a time the ideal lattice was the central object of study, we begin to-day to understand the reasons why actual crystals do in many ways deviate from this ideal pattern.

Many of these investigations are independent of a detailed knowledge of the atoms themselves, using only some crude averages of their geometrical and dynamical properties, like diameter, charge, dipole moment, polarizability, etc.

The problem that remains is to understand these averages; that means, to investigate the nature of the atoms themselves.

The research in the structure of the atom is intimately connected with radioactivity. The discovery of radioactivity belongs to the nineteenth century. Its rapid development is mainly due to one man—Lord Rutherford. He demonstrated the atomistic character of the α- and β-radiation by counting the particles, using first the scintillation method of Crookes (1903), later the Geiger counter (1908). In the later development of counting methods a decisive factor was the amplifying electronic valve, invented in its simplest form (diode) by Fleming in 1904 and improved (triode, pentode, etc.) by de Forest in 1907 and Langmuir in 1915.

Let me mention here some other experimental techniques of great importance which enable us not only to count but also actually to see the tracks of particles: C. T. R. Wilson's cloud chamber (1911) and its refinement by Blackett (1937), the counter-controlled cloud chamber. Then the method of photographic tracks discovered by Blau and Warmbacher in 1937, which through the improvement of emulsions has become a most efficient tool for studying atomic processes.

The first revolutionary results, obtained with the then available primitive experimental technique, by Rutherford and Soddy about 1900, were the laws of radioactive disintegration which shattered the belief in the invariability of the chemical elements. These laws differ from the ordinary deterministic laws of classical physics, being intrinsically statistic and indeterministic.

At the same time ample proof for the existence of isotopes was found among the radioactive elements. Later, in 1913, J. J. Thomson discovered the first example of isotopy among ordinary elements (neon) by electromagnetic deflexion. From here came on one hand Aston's mass spectrograph (1919), the renewal of Prout's hypothesis and the modern version of the Periodic Table with its arrangement of the atoms according to nuclear charge (atomic number Z) as opposed to mass (mass number A); on the other hand, the separation of isotopes in bulk as performed to-day on an industrial scale for the production of fissionable material.

The distinction between these two numbers Z and A is mainly due to Rutherford's second great discovery (1911), the nucleus, obtained through the observation of scattering of α-rays. The result that Coulomb's law is valid down to nuclear dimensions suggested to Rutherford the planetary model of the atom, with the nucleus in place of the sun, and electrons in place of the planets. A welcome confirmation of this was soon (1913) provided by Moseley with the help of X-ray spectra. But formidable theoretical difficulties arose because of the lack of stability of such systems according to the laws of classical mechanics.

In fact, atomic research had reached here a point where progress was not possible without a radical change of our fundamental conceptions.

This revolution of thought was already in progress. It had started in 1900, just at the beginning of this period of review, when Planck convinced himself that the observed spectrum of black bodies could not be accounted for by classical mechanics, and put forward the strange assumption that finite quanta of energy ε exist which are proportional to the frequency, $\varepsilon = h\nu$.

The physical world received this suggestion with great scepticism as it did not fit at all into the well-established wave theory of light. Years passed without much happening. But in 1905 Einstein took up Planck's idea and gave it a new turn; he showed that by assuming the light to be composed of particles, later called photons, a quantitative explanation of the photoelectric effect in metals and of similar phenomena is obtained. Using Einstein's interpretation Millikan (1910) derived from measurements of the photo-effect a value of h in excellent agreement with Planck's original one.

Further evidence for the existence of quanta was given again by Einstein in 1907 through his theory of specific heat, mentioned already, which not only removed some very disquieting paradoxes of the kinetic theory, but also served as a sound foundation of the modern theory of molecules and crystals.

The final triumph of quantum theory was Bohr's application to Rutherford's planetary model in 1913. It solved the riddle of atomic stability, explained the mysterious spectral series and the main features of the periodic system.

Bohr was, right from the beginning, quite clear that the appearance of the quantum meant a new kind of natural philosophy, and so it has turned out. Yet at the same time Bohr was anxious to keep the connexion with classical theory as close as possible, which he succeeded in doing with the help of his principle of correspondence.

There followed a period of about twelve years in which Bohr's ideas were confirmed and developed. Here are a few outstanding events:

Franck's and Hertz's experiments to demonstrate the existence of stationary states with the help of electronic collisions (1914). The disentanglement of the multiplet spectra, including X-rays, by numerous authors, theoretically guided by Bohr and Sommerfeld. Landé's formula for the Zeeman effect (1921) which led finally to the suggestion of the spinning electron by Uhlenbeck and Goudsmit (1925). The confirmation of Sommerfeld's "quantization of direction" by Stern and Gerlach (1921). The refinement of the theory of the periodic system by Bohr himself, confirmed at once by the discovery of one of the missing elements, hafnium, by Coster and Hevesy (1922). Then—most important—Pauli's exclusion principle (1924), which gave a theoretical foundation to important features of observation. Finally, the Compton effect (1923), which demonstrated the usefulness of Einstein's conception of photons.

Thus the paradoxical situation had to be faced that both the undulatory and the corpuscular theory of light were right—in fact, Planck's formula $\varepsilon = h\nu$ states a relation between these contradictory hypotheses.

This challenge to reason came to a climax through de Broglie's famous thesis of 1924 in which this duality wave-corpuscle was, by a purely theoretical argument, extended to electrons. The first confirmation was given by Elsasser (1927) with the help of experiments on electron scattering on metals made by Davisson and Germer (1927), and soon these authors, and independently G. P. Thomson, the son of the discoverer of the electron as a particle, produced diffraction patterns with metal foils which established definitely the existence of de Broglie's waves.

May I mention here in parenthesis that the idea of the electron microscope is considerably older than this theory; it was first suggested in 1922 by H. Busch on the grounds of considerations analogous to geometrical optics. After de Broglie the wave theory of optical instruments became applicable and the resolving power could be determined. I cannot dwell on details, but I wish to remind you that

to-day not only bacteria and viruses but even big molecules can be made visible and photographed.

The duality wave corpuscle made an end of the naïve intuitive method in physics which consists in transferring concepts familiar from everyday life to the submicroscopic domain, and forced us to use more abstract methods.

The first form of this new method was mainly based on spectroscopic evidence which led Kramers and Heisenberg to the conviction that the proper description of the transition between two stationary states cannot be given in terms of the harmonic components of these states separately, but needs a new kind of transition quantity, depending on both states. Heisenberg's quantum mechanics of 1926 is the first formulation of rules to handle these transition quantities, and these rules were soon recognized by myself as being identical with the matrix calculus of the mathematicians. This theory was developed by Heisenberg, Jordan and myself, and independently, in a most general and perfect manner, by Dirac.

Again, independently Schrödinger developed in 1926 de Broglie's wave mechanics by establishing a wave equation valid not only for free electrons but also for the case of external fields and mutual interaction, and showed its complete equivalence with matrix mechanics.

Concerning the physical interpretation, Schrödinger thought one ought to abandon the particle conception of the electron completely and to replace it by the assumption of a vibrating continuous cloud. When I suggested that the square of the wave function should be interpreted as probability density of particles, and produced evidence for it by a wave theory of collisions and other arguments, I found not only Schrödinger in opposition, but also, strangely enough, Heisenberg. On the other hand, Dirac developed the same idea in a mathematically brilliant way, which was soon generally accepted, also by Heisenberg who produced a most important contribution by formulating his uncertainty relations (1927). These paved the way for a deeper philosophical analysis of the foundations of the new theory, achieved by Bohr's principle of complementarity, which replaces to some degree the classical concept of causality.

In a very short time the new theory was well established by its successes. I can mention only a few points: Pauli's matrix representation of the spin and Dirac's relativistic generalization (1928) which led to the prediction of the positron, actually found by Anderson in 1932. Then came the systematic theory of the electronic structures of atoms and molecules and their relations to line and band spectra, to magnetism and other phenomena. Wigner showed in 1927 how the general features of atomic structures could be found with the help of group theory. Hartree, Fock, Hylleraas and others developed numerical methods. The theory of collisions of atoms with electrons and other atoms was started by myself and developed by Bethe, Mott, Massey and others, from which finally sprang a general theory of the penetration of particles through matter by Bohr.

Further, the derivation of the nature of the chemical bond, initiated by Heitler and London in 1927, was worked out by Hund, Slater, Mulliken, Pauling and others. Even the complicated phenomena of reaction velocities, including catalytic acceleration, have been reduced to quantum mechanics.

Finally came Dirac's most important theory of emission, absorption and scattering of electromagnetic radiation which led to the first systematic attempt of formulating quantum electrodynamics by Fermi, Jordan, Heisenberg and Pauli (1929), and later to the general theory of quantized fields and their interaction (Wentzel, Rosenfeld, from 1931).

The last period of our fifty years is dominated by nuclear physics. Although the importance of nuclear research is probably greater than that of any other branch of physics, I shall be rather short about it for it is the most recent phase of our science and scarcely yet history.

The first breaking up of a nucleus was achieved by Rutherford in 1919, by bombarding nitrogen with α-rays. Artificially accelerated particles were first used by Cockcroft and Walton in 1930. At that time the nucleus was believed to be composed of protons and electrons. But this led to difficulties if one tried to derive the angular momenta of nuclei from the spins of the component particles. In 1932 Chadwick discovered the neutron, and those difficulties disappeared if the nucleus was assumed to consist of protons and neutrons, or charged and uncharged 'nucleons'. Fermi showed in 1932 that neutrons are most efficient in disrupting nuclei as they are not repelled by the nuclear charge. Many of the residual nuclei were found by Irene and Frederic Joliot-Curie in 1934 to be radioactive themselves.

The continuous β-ray spectrum offered great difficulties to the understanding until Pauli, in 1931, suggested the existence of the neutrino and Fermi developed, in 1934, the neutrino theory of the β-decay where the laws of conservation of energy and momentum are preserved. The line spectrum of β-rays was recognized to be of secondary origin, namely, due to the expulsion of electrons from the electronic cloud by γ-rays emitted by the nucleus.

The need for fast projectiles was first supplied by the use of cosmic rays. These had been discovered by Hess already in 1912 and their study has grown to-day into a vast science, covering not only nuclear physics but also geophysics, astronomy and cosmology.

The artificial production of fast particles has made enormous strides through the construction of powerful accelerating machines, as that of van der Graaf (1931), Lawrence's cyclotron (1931), Kerst's betatron (1940) and combinations of these, like the synchrotron.

The clue to the interpretation of nuclear transformations is Einstein's formula $E = mc^2$, or more precisely, the relativistic conservation laws of energy and momentum. I am not an expert in the new awe-inspiring science of nuclear chemistry and shall make no attempt to describe it. I can say only a few words on the theoretical problems of nuclear physics. It is remarkable how many important facts can be understood by extremely simple models, as, for example, Gamow's crater model (1928), which explains the α-decay and the Geiger–Nuttal relation between α-energy and life-time; and the liquid-drop model, suggested by von Weizsäcker in 1925, to explain the mass defect (nuclear energy) curve and later used successfully by Bohr (1935) to explain the mechanism of capture, re-emission and fission. A great amount of work has been done on exact quantum-mechanical calculations of the structure and properties of light nuclei (in particular the deuteron) and of the effect of collisions, with the aim of learning something about the nuclear forces. Important results have been obtained, but altogether the situation is not satisfactory.

Quite independently of detailed theories, the empirical values of the nuclear masses (internal energies) indicate that the light nuclei have the

tendency to fuse, the heavy ones to disintegrate; hence all matter, except the elements in the middle of the periodic system (iron), is in principle unstable. But reaction velocities are, under terrestrial conditions, so extremely slow that nothing happens. It is, however, different in the interior of stars; Bethe showed in 1938 that one can account for the heat developed in the sun and the stars by a nuclear catalytic chain reaction, the fusion of four nucleons to form a helium nucleus.

The opposite phenomenon, the fission of the heavy nucleus of uranium into almost equal parts, discovered by Hahn and Strassmann in 1938, has initiated a new era in the sociological situation of our science and very likely in the history of mankind. Here is a list of events:

The establishment in 1939 of the possibility of a self-supporting chain reaction by different authors (Joliot, Halban and Kovarski; Fermi; Szillard); the construction of the first nuclear reactor or 'pile' under the direction of Fermi in 1942, and, finally, the harnessing of the industrial power of the United States to produce the atom bomb.

The political and economic implications of this development are too formidable to be discussed here; but I cannot refrain from saying that I, personally, am glad not to have been involved in the pursuit of research which has already been used for the most terrible mass destruction in history and threatens humanity with even worse disaster. I think that the applications of nuclear physics to peaceful ends are a poor compensation for these perils.

However, the human mind is adaptable to almost any situation. So let us forget for a while the real issues and enjoy the useful results obtained from the pile. In physics the remaining few gaps of the Periodic Table have been filled and five or six transuranium elements (among them fissionable nuclei like neptunium and plutonium) discovered. Innumerable new isotopes of known elements have been produced. Some of these can be used as 'tracers' in chemical and biological research, as first suggested by von Hevesy in 1913; others as substitute for the expensive radium in industrial research and in the treatment of cancer.

From the point of view of natural philosophy the most important achievement of the past decade seems to me the discovery of the meson, theoretically predicted by Yukawa in 1935, which showed how far we are still removed from a knowledge of the real fundamental laws of physics. Yukawa became convinced that the forces between nucleons are at least as important as the electromagnetic forces, and by applying the field concepts in analogy to Maxwell's theory was able to predict a new particle which has the same relation to the nuclear field as the photon to the electromagnetic field, but has a finite rest-mass, which from the range of nuclear forces could be estimated to be about 300 electron masses. Soon the existence of mesons was experimentally confirmed in cosmic rays by Anderson and Neddermeyer in 1936 and later with particles produced by the cyclotron in California in 1948. The method of photographic tracks has, in the hands of Powell (from 1940) and others, produced a wealth of new results, for example, the spontaneous disintegration of the meson of about 300 electron masses into a lighter one of about 200 electron masses and a neutral particle. A meson of about 900 electron masses has been fairly well established, and it is not unlikely that still more types exist.

It is obvious that to understand all this a much deeper research in the theory of quantized fields and their interaction is necessary. A revised and modernized quantum electrodynamics was published independently by Schwinger in the United States and Tomonaga in Japan in 1947, and from this has sprung a considerable amount of literature, aiming at the elimination of divergence difficulties and calculating effects of higher order, inaccessible to the older theory. A great success was the explanation of an observation made by Lamb and Retherford in 1947, which showed that Dirac's celebrated theory of the hydrogen spectrum is not quite correct. But it becomes more and more clear that all these mathematical refinements do not suffice, and that a far more general theory has to be found, in which a new constant (an absolute length—or time, or mass) appears and which ought to account for the masses found in Nature. I wish to end this outlook into the future with a remark I have recently heard from Heisenberg. We have accustomed ourselves to abandon deterministic causality for atomic events; but we have still retained the belief that probability spreads in space (multi-dimensional) and time according to deterministic laws in the form of differential equations. Even this has to be given up in the high-energy region. For it is obvious that the absolute time interval restricts the possibility of distinguishing the time order of events. If this interval is defined in the rest system it becomes large in a fast-moving system according to the relativistic time expansion (in contrast to the contraction of length). Hence the indeterminacy of time order and therefore of cause–effect relation becomes large for fast particles.

Thus experience again leads us to an alteration of the metaphysical foundations of a rather unexpected kind. In fact, traditional philosophy has provided the leaders of our science, like Einstein, Bohr and Heisenberg, with problems in so far as it failed to supply answers agreeing with experience. I am convinced that although physics free from metaphysical hypotheses is impossible, these assumptions have to be distilled out of physics itself and continuously adapted to the actual empirical situation. On the other hand, the continuity of our science has not been affected by all these turbulent happenings, as the older theories have always been included as limiting cases in the new ones. The scientific attitude and the methods of experimental and theoretical research have been the same all through the centuries since Galileo and will remain so.

PHILOSOPHY OF PHYSICS, 1850–1950

By Prof. H. DINGLE

THE history of physics is recorded in the publications of the recognized scientific bodies; it is there to see, and cannot be altered. Also, except for the most recent part of our period, there is little doubt and no difference of opinion concerning what is most important in that history; our best text-books differ only slightly, if at all, in what they select as now surviving out of the vast output of the past century. Consequently, in listening to Prof. Andrade and Prof. Born we have been listening almost exclusively to the authentic voice of physics itself.

In the nature of the case, my credentials are less firmly established. The published documents to which I can refer scarcely exist: the *British Journal for the Philosophy of Science* first appeared in May

1950. There were, it is true, isolated writings on the subject throughout the century—mostly from men who, having deservedly acquired a high reputation for their contributions to physics, were more or less forced by popular demand to try to interpret those discoveries in philosophical terms. The results in most cases have fallen into the oblivion they deserve. Reading them is something like listening to a spider discoursing on civil engineering or a seagull formulating the principles of aerodynamics. The unfortunate consequence is that in order to give an account of the philosophy of physics during a period of unexampled physical progress I have to depend very largely on what I can deduce from the implications rather than the direct statements of the writings of the time.

Nor does this exhaust my difficulties. It is not easy to express, with the rigour expected of Section A in the year A.D. 1951, even what is meant by the phrase 'the philosophy of physics'. What I propose to do, therefore, is to give a general idea of what I mean by it. By the philosophy of physics, then, I shall mean the general outlook on experience that one adopts, whether consciously or unconsciously, in terms of which he automatically interprets the discoveries and generalizations of physics and gives them meaning. It is the wider framework of conceptions or instinctive beliefs into which one fits the state of physics at any particular time. One may, in principle at least, form such a philosophy in either of two ways. The first is in terms of what I may call *a priori* notions to which one makes the progress of physics conform. Thus, for example, one may start with the common-sense outlook—what philosophers call *naïve realism*—or with a view of the nature of things fundamentally determined by his religious experience and what seems to him its most natural rational expression, or with the supposed laws of movement of society resulting from economic forces, as the Marxists do, or with some other non-physical set of principles, and look on the successive achievements of physics as completing in one direction the description of the world which that philosophy demands. The other possibility is that of ridding one's mind so far as possible of all such preconceptions, taking the world-picture suggested by physics as one's starting-point, and generalizing it to form the all-comprehensive world-picture into which it most naturally fits.

Until recently, as intellectual history goes, the former alternative was the only possible one. True, the subject-matter of physics—the elementary awarenesses of movements, of heat and cold, of sound and sight, and so on—were among the earliest and most immediately noticeable elements of our experience; but for that very reason it was imperative to attain to a purely practical, rather than a strictly rational, understanding of them in order merely to survive. The luxury of calm, philosophical contemplation could be indulged best in those realms now given over to psychology and biology and even to phenomena scarcely yet generally recognized as ripe for scientific exploitation—with the result that of what we now call physical laws of Nature, nothing was known before the seventeenth century, and what was thought to be known was but a subsidiary element of a great world-scheme that owed its primary inspiration to other things. But now the situation has changed. The new attitude to philosophy which was inaugurated by Galileo and developed by Newton has spread so rapidly and brought within a few general principles such a vast body of physical experience that no one, whatever his predilections, can fail to recognize that it forms by far the largest and most comprehensive homogeneous body of rationally organized knowledge that we possess. If a stranger to our planet and to our whole type of experience were suddenly endowed with the power of observation and fully developed rational thought, and were to examine the systems of knowledge which we have through the ages succeeded in building up, he could not fail to notice that in magnitude, in coherence, and in the certitude that comes from general agreement, the physical scheme is overwhelmingly ahead of all others. If he wished to obtain a complete rational co-ordination of his experience he would inevitably choose physics as his starting-point, and pursue the path through physics to philosophy.

This fact, however, was only just beginning to be realized about a hundred years ago; it is far from being generally realized yet. During the Middle Ages the predominant world-picture was compounded of elements drawn from Greek philosophy and speculation and Christian dogma, and into that picture such discoveries as were made were fitted without fatal distortion. The scientific revolution of the early seventeenth century coincided with a philosophical revolution, led independently by Bacon and Descartes, which resulted in the substitution for the medieval scheme of what may be called common-sense philosophy, in that it took for its fundamental dogma the Cartesian division of the object of investigation into extended things and thinking things, or, briefly, matter and mind. Of these, when Galileo and Newton had shown the way, matter was relatively easy to study, mind almost impossible. The result was that the physicists, who were the successful explorers of Nature, tended to leave mind out of the field of things to be investigated and to regard it simply as the means of investigation. The world, then, was the world of matter—or of *substance* as, by adoption of a scholastic term, it came to be called. Its constituents moved about in space and time, which accordingly could, in a sense, be called a part of the world, though not a part needing any particular attention. Euclid had once for all formulated the laws of space, and time was obvious enough, so one could devote his whole attention to the nature of matter and the laws of its behaviour. The so-called 'scientific method' of Galileo and Newton was, as its name implied, a means of studying this material or substantial world. It had at its command two instruments—observation, in the form of experiment (that is, observation under controlled conditions), and reason, usually in the form of mathematics.

This philosophy, as I say, was a common-sense philosophy. It was derived, not from a study of the practice of Galileo, Newton and their successors, but from the age-long common-sense belief in an 'external world', that 'existed' whether it was observed or not, and that caused observations by acting in one way or another on the sense organs of the observer. The Cartesian dualism was conformable to that belief, which accordingly took on the dignity of a philosophical system but was none the less a simple naïve realism, and so far as men of science were concerned it was for the most part held unconsciously rather than openly acknowledged. Throughout the eighteenth and early nineteenth centuries, men of science were doing things. The question what exactly it was that they were doing did not disturb

them; but if asked they would have said they were discovering the contents and laws of behaviour of the external world. They would have been perfectly honest in saying this, and most of them, in fact, would have been astonished if told that any other description of their actions was possible; but it was not a belief arrived at from an actual examination of those actions. The prior existence of the external world was accepted as unquestionable, apart altogether from the practice of science. 'Scientific method' was only a particular way of learning its structure and behaviour—the best way, of course, and perhaps the only trustworthy one, but still just one among others. What physicists were discovering had to be fitted into that framework, and up to the year 1851 at least it found a natural place there without the slightest distortion.

It was roughly about this time, however, that the first explicit attempts to formulate a philosophy of science, starting from science itself, became discernible. I believe the first book to take this as its avowed object was Whewell's "The Philosophy of the Inductive Sciences", which was published in 1840. True, there is little enough in the philosophy of this book that would not have been there had the author been writing as a philosopher of the traditional type rather than as a man of science; but the way in which the ideas are presented does take its shape from the necessity to accommodate the physical science of the time, and one can almost see the author's pen being unconsciously guided by the impulse to exhibit as eternal truth the picture of the world that justified most indubitably the prevailing state of physics. And, indeed, it was not difficult to do this. The Newtonian ideal—that of ascribing all phenomena to precisely specifiable forces acting on a perfectly inert substance according to strictly causal laws—had been uninterruptedly in course of realization; it fitted the Cartesian philosophy so far as material substance was concerned, with the slight difference that the fundamental property of matter, which Descartes had taken to be extension—the power of occupying space—was now thought to be mass; and the method of induction from experiment, which was the hallmark of science, had established its value almost beyond reasonable expectation. One could therefore pursue the scientific adventure with complete confidence that it not only conformed to the accepted principles of philosophy, but even confirmed and exemplified those principles.

The subsequent history of physics is the story of the uprooting of that confidence. There are two broad aspects of this process, corresponding to the physical progress that has culminated in quantum theory and in relativistic field physics, respectively, but before describing the effect of these lines of research I ought to mention that, quite apart from the further development of physics itself, the essential inadequacy of the current philosophy was at least suspected by a few physicists because of its unsatisfactory foundations. Newtonian mechanics, for example, indubitably worked, and was able not only to account for the phenomena that came within its scope but also to make predictions of new phenomena which were duly verified—the discovery of Neptune is a case in point; but the fundamental equations of motion, on which the whole scheme rested, involved logical difficulties that could not be regarded without uneasiness by logical minds. Newton's absolute space, for example, could not be clearly conceived, and there were difficulties in framing the second law of motion in a form that did not include a circular argument. Moreover, as the century progressed, the contradictory properties that had to be assigned to the ether to enable it to fulfil its various functions, culminating in those laid on it by Maxwell's electromagnetic theory of light, led some physicists to doubt whether the simple philosophy that represented physics as an objective study of a given external material world was altogether justifiable. Such men as Kirchhoff, Hertz, Mach, Pearson, Poincaré, in their various ways, gave voice to these doubts and expressed the view that the achievements of physics were not properly represented as the discovery of the nature of an external world. The world they were supposed to be elucidating looked irrational, and they did not wish to study the irrational. Accordingly, they sought a re-formulation of the aims and practice of physics that would make sense of what they and their colleagues were doing. But all this had little effect on the progress of physics as a whole. The building was going up and the parts were fitting together perfectly: why bother about foundations? Let the work proceed and leave the first principles to be justified in time by those who cared for such things. The important thing was to press forward, not to trouble one's self about the point at which the process began.

So it came about that the overthrow of the nineteenth-century philosophy of physics was left to be brought about by the development of physics itself. It would scarcely be true to say that the building collapsed. Rather, a point was reached at which progress stopped. Further bricks were coming to hand, but they could not be fitted into the existing structure without disaster; the foundations were not firm enough to support them. As I have said, the trouble arose in two distinct departments of physics—particle physics and field physics, respectively. I can, of course, give only an outline of what has happened, and I will take the two departments separately, though historically their development overlapped. To put the matter briefly, the undermining of the older philosophy came about gradually in particle physics, beginning with the reduction of matter to elementary particles, the acceptance of which became established doctrine about the beginning of our period, and ending with quantum mechanics, which is one of the highlights of its end. Relativity theory, on the other hand, gave that philosophy a sudden death-blow about the middle of the period. Let me take the gradual process first.

The kinetic theory of matter sought to explain all the manifest properties of bodies in terms of the movements of ultimate indivisible and unalterable particles obeying the Newtonian mechanical laws. What particular forces acted on them remained to be found out, but that some strictly specifiable forces did act, to which they were in absolute subjection, was taken for granted, and the mass movements, colours, temperatures, chemical reactions, and all the rest of the sensible reactions of bodies that make up our apprehension of the world, were believed to be derivable from the mechanical behaviour of the molecules, as they were usually called. These molecules were the reality; what was observed was only appearance, and the problem of physics thenceforth was to find out all that could be discovered of the reality.

This could be done, of course, if one could discover the forces acting on the molecules, and the positions and motions of the molecules at some single instant.

Since these things could not be observed, however, they had to be arrived at by inference, and the way to discover them was anything but clear. But a most important discovery was made. It was possible to describe many of the properties of bodies—and especially gases—in terms of molecules without, in fact, knowing anything at all about the behaviour of any single one of them, except that it possessed a few conceptual properties such as motion, momentum, elasticity. Curiously enough, although the molecules were conceived to be the ultimate reality of the material world, they were at first denied the fundamental Cartesian property of matter, namely, extension; they were conceived as Euclidean points. This was later rectified when it was found that the assumption of definite dimensions extended the range of usefulness of the theory, but it affords an interesting illustration of the essential indifference of physicists to philosophical requirements and exemplifies the fact, which I mentioned before, that a highly abstract conception like mass had displaced a manifest quality like extension as the fundamental characteristic of matter.

The result of this discovery was that the development of kinetic theory took a statistical form and was directed to the interpretation of observations (that is, appearances) in terms of the collective behaviour of multitudes of conceptual molecules. Now it is not difficult to see that this was a betrayal of the true mission of physicists according to the accepted philosophy. They were dedicated to the investigation of reality, and that had become the investigation of the nature and behaviour of the molecules; and instead of pursuing that they occupied themselves in showing how their ignorance of reality could be used in order to describe mere appearances. They could account for the temperatures of bodies, their pressures, viscosities, thermal conductivities, and so on—properties which had no meaning when applied to individual molecules but were significant only with regard to groups of many millions of them; and they were pleased with these achievements. What they were doing was finding rational relations between their observations, and using the molecules merely as counters, as dummies, as useful conceptions by means of which to extend the scope of those relations. And so indifferent were they to the incompatibility of their practice with their philosophy that they were for the most part quite unaware that they were doing anything irregular at all. To give an account of the gas laws in terms of molecular movements was a great triumph. If anyone had said: "But we knew those laws before in a much simpler form: you have merely made them abstruse", they would have replied: "No; we have shown what really goes on in the gas". If then their critic had said: "Well, what does really go on? What is the nature of these molecules? Where is any one of them at any particular moment, and how does it move afterwards? Why do the molecules behave as they do and not otherwise?", the reply would have been, "Well, we don't know those things yet, but no doubt we shall some day". If the critic had then persisted: "So what you have actually done is to find a cumbersome, but possibly fruitful, way of expressing relations between appearances, and left reality to look after itself", they would no doubt have said: "Sir, I perceive that you are a philosopher; we are physicists".

The matter, of course, came to a head when means really were found of investigating the nature of individual molecules. They were reduced first to atoms, then to what are called 'fundamental particles'—electrons, protons, neutrons, and a good many more; the number is indefinite. These are now the ultimate realities. And what do we know about them? Nothing. Not only so, but we believe that we have proved that we *can* know nothing. The utmost we can do is to calculate the probability that one of them will be at a given place at a given time. Of the picturable properties that one would expect small bits of matter to have they have none at all. We cannot even count them—that is to say, not that the task is too difficult, like that of counting the stars in a stellar system, but that they do not admit of being counted; they have not the individuality that formerly would have been regarded as so essential a property of a real particle as to be more fundamental than even extension or mass. We can make them serve our purpose of describing the relations between phenomena only by making them obey statistical laws impossible with objects that can be counted individually. We nevertheless do this, and thereby acknowledge the fact that we are no longer interested in 'reality' so long as we can find rational relations between our observations. We are satisfied to say that a particle, once the ultimate reality of the universe, has no individuality, may be anywhere at all, though it is more probably here than there, and has a future indeterminable from our present knowledge, for it observes no causal laws. The pursuit of reality has ended as a greyhound race ends, with the disappearance of the hare underground. And the only intelligible conclusion is that the object of the whole business is not to catch the hare but to run the race. The only philosophy that accords with the whole history of physics is that which represents physics as the establishment of rational relations between phenomena, not the penetration of phenomena to reach a hidden reality that causes them.

Let me turn now to relativity theory. I am, of course, not going to describe the steps that led to it, but only to consider the nature of the problems involved. In accordance with the philosophy of the time, the real material world, whether regarded as consisting of molecules or of gross bodies, was conceived to possess its properties by intrinsic right. Thus its constituents had a size, a mass, a velocity, and so on. These properties could, in principle at least, be determined; one of the objects of physics, in fact, was to determine them as completely as possible. But even if the attempt to determine them was unsuccessful, they unquestionably existed. The mass of a body was such and such, whether we knew it or not; its length was such and such, whether we knew it or not. Now the basic requirement of the theory of relativity was that all these properties were almost completely indefinite. Take any object—a stick of chalk, for example. Its length is, say, two inches or anything at all less than that, according to the velocity which it has, and that velocity can be held to be anything from zero up to the velocity of light. The higher the velocity, the smaller the length, and no one velocity can be said to be more really correct than another. One may be more convenient than another in a particular problem, but it will be less so in a different problem; fundamentally, the chalk may be said to have any velocity at all between the limits I have mentioned, and no one value is truer than another.

Now it happens that all our measurements depend in one way or another on the measurement of length,

and the result is that *all* physical properties of a body depend on this arbitrary velocity. Its mass, for example—the fundamental property of the materialistic philosophy—may be anything from a few grams up to infinity. There is no single quantity that is not almost infinitely arbitrary, having no assignable value at all until you have decided how the body is to be regarded as moving, and the choice of that lies entirely with you; Nature has nothing whatever to do with the matter.

This again is now accepted physical belief. It is not something imposed on physicists from without, but something unavoidably arrived at by them in the normal exercise of their practice. In particle physics we saw that while we were learning less and less about the nature of the fundamental particles, we were learning more and more about the relations between phenomena by using the particles as terms of expression. In relativity theory we see similarly that by abandoning all attempts to assign any definite property at all to matter we can learn more and more about the relations between phenomena. We succeed in the practice of physics in the same measure as we fail to honour the supposed philosophy of physics.

There are two ways of dealing with this situation. One is to retain the old preconception that we are investigating the nature of a real, external, material world, and to add that we have discovered that it is essentially mysterious and even self-contradictory. If you do retain that preconception the addendum is, indeed, inevitable; the world *is* mysterious and self-contradictory. Personally, I find this a very unsatisfying view. Not only does it end in complete nescience, but also it takes no account of the very great and positive achievements of physics. I prefer to take those achievements as indicating the true nature of physical investigation, and that means that we must regard physics as the progressive establishment of rational relations between the results of our measurements. From this point of view the quantities with which physics concerns itself are not evaluations of objective properties of parts of the external material world; they are simply the results we obtain when we perform certain operations. They are just as objective, of course, in the sense that we have no ultimate control over them: we perform the operation and get the result that comes, not the result we may wish. But we do not add the unnecessary and, as it turns out, frustrating corollary that they are measurements of a pre-existing material world of which in its essence we can never know anything.

On the whole, the tendency of those who have attempted to formulate a philosophy of physics in the light of the new knowledge has been to combine these alternatives. They have, in the main, had to recognize the logical force of the second, but have been unwilling or unable to give up a belief that has seemed so imperative as to be beyond denial. I have time for only one or two examples.

The late Sir Arthur Eddington is an outstanding one. His tragedy was that he was born too soon and possessed too precocious and retentive a mind. Consequently, the belief that physics was the examination of the external world became so fixed in his consciousness before the quantum and relativity theories arose that, although he immediately recognized their fundamental importance, he was forced to adapt them to beliefs with which they were, in fact, incompatible. Thus, in 1923, in his "Mathematical Theory of Relativity", we find him saying, and even italicizing the statement, that "*a physical quantity is defined by the series of operations and calculations of which it is the result*". There was really nothing more to be said about it; but for the reason I have mentioned he could not refrain from adding that it represented what he called a "world-condition", which was, however, essentially unknowable. The mysteriousness of these world-conditions was emphasized by an example. The distance of a star and the parallax of a star are two physical quantities; but they are so related that they are used alternatively in all our calculations. They therefore, he concluded, represent the same world-condition. But now, if one of two stars has twice the distance of the other it has half the parallax. Is then the world-condition of the first twice or half that of the second, or is any metrical relation between them inconceivable? We do not and cannot know. The world which has these conditions was, according to Eddington, "the mysterious background of phenomena". He could not see that it was precisely the same kind of thing as the "mere names", according to Galileo, or "occult qualities", according to Newton, that flourished in medieval thought and, as Newton expressed it, "put a stop to the improvement of natural philosophy".

About the same time as Eddington was formulating these views the movement that has become known as logical positivism originated on the Continent through the formation of the Vienna Circle and, later, the Verein Ernst Mach, the object of which was to cleanse philosophy of all those elements that the development of physics had shown to be hindrances. In some respects this movement ran parallel to the movement of Eddington's thought; in others, owing to its entirely different background, it took a quite different course. Thus Eddington asserted that only through physical quantities could we represent to ourselves a condition of the world. "Any attempt to describe a condition of the world otherwise," he wrote, "is either mathematical symbolism or meaningless jargon." He accordingly recognized three types of ostensible approach to a description of the world—physical investigation, mathematical investigation, and meaningless chatter. The logical positivists sought precision by avoiding all reference to operations and speaking in terms only of the sentences in which those operations and their results were expressed, but they reached essentially the same three-fold classification; they divided all possible word sequences into synthetic statements (that is, those expressing the results of sense observations), analytic statements (those occurring in pure mathematics, which were regarded as tautologies), and nonsensical statements. The parallel with Eddington is complete; but whereas Eddington in his classification restricted himself wholly to the field of physics and did not attempt to say anything about other realms of experience, the logical positivists applied their principles to sentences about anything at all, and were accordingly led to describe every statement about the subject-matter of metaphysics, normative ethics and (except in so far as it could be reduced to physics) psychology as nonsense. I mention this merely to prevent misunderstanding; but it is not our present subject, and, returning to physics, we may say that Eddington and the logical positivists were very largely in agreement on the implications of its later developments. So far as the external world was concerned, however, they were less tied to

tradition than he was, though they seem to have been unable wholly to disregard the traditional way of speaking. Formally they recognized the external world merely as a convenient form of expression, though the convenience became more obvious the further one removed one's self from physics, but when they were not actually discussing the question of its status they accorded it much more than the homage due to mere convenience. They insisted, for example, that the only kind of statements that "tell us anything about the world" are synthetic statements, and one does not easily find any characterization of such statements from which reference to "the world" is excluded.

I will not follow the later developments of logical positivism, which seem to have emphasized the positive rather than the logical side of the programme, because, in spite of much that is valuable in this movement, I think it suffers from a dearth of scientific men and a surplus of philosophers. I say this not with any provocative intention; but simply because it is a fact, and one which shows itself very plainly in the course the movement has taken. The logical positivists are concerned chiefly with the 'verification' of sentences, and take great pains to prescribe as precisely as possible what steps have to be taken to ensure such verification. In other words, the sentence itself is their starting-point, and its verification (or falsification) their goal. But the sentences with which they start are not the starting-point but the goal of the physicist. His problem is to proceed from evidence to conclusion, not to start with a 'conclusion' and prove it. Proof may be impossible, for the observations that constitute the evidence may not be repeatable; but the physicist still has the problem of making deductions from them. Hence, whatever may be called verification in such a case may be essentially different from that relevant when repetition is presumed to be possible.

The logical positivists, furthermore, emphasize the unity of science, by which they mean the thesis that all science is reducible to physics. To the men of science this appears to be an arbitrary dogma. They establish it simply enough by characterizing all sentences that cannot be expressed in the existing physical or mathematical language as nonsensical. The scientist, it is true, may look forward to the unity of science, but he cannot adopt it as a presupposition; it is something to be, if possible, achieved. For the moment he is content to proceed in the different sciences on independent lines, using independent means of observation and independent concepts. He may hope that in time there will be a synthesis of the physical, biological and psychological sciences, just as the originally independent sciences of mechanics and heat have merged into thermodynamics, and magnetism and electricity into electromagnetism. But that is a hope, not a categorical pronouncement, and the concepts of the unified science may be biological or psychological rather than physical, or possibly concepts which are entirely new.

The most satisfactory statement of the philosophy of physics to which the later developments point seems to me to have been begun by Prof. P. W. Bridgman. Like Eddington he regards all physical quantities as the results of operations, but he does not encumber his thought with the useless world-conditions that Eddington had to drag behind him in all his adventurings. Bridgman's work has lain mainly in a progressive revision of all physical concepts in order to ensure that they represent no more and no less than particular precisely describable and practicable operations. The necessity for this work can be realized when it is remembered that the whole theory of relativity arose from the discovery that the concept of absolute simultaneity of events at different places corresponded to no practicable operation. In one respect, however, I think he goes too far, and in another not far enough. He applies his analysis to all physical concepts, whether they are employed as the terms in which physical results are expressed or only as instruments to facilitate the attainment of those results. For example, he examines equally the operational meaning of the concept of viscosity and the concept of action at a distance, and is prepared to brand either concept as illegitimate if the corresponding operation is not discoverable. I admit the necessity for this so far as viscosity is concerned, for that is represented by a symbol in physical equations, and the meaning of those equations is indefinite so long as the operations of which its terms are the products are unknown. But action at a distance is not represented by a symbol in any physical equation. It is merely a concept which enables us to picture the situation represented by the equation in such a way as to suggest what next to do. It may fulfil this function whether it corresponds to an actual operation or not, and since, having discarded the external world, we are in no danger of thinking it the 'truth' about Nature, I see no reason for proscribing it if it serves our purpose.

On the other hand, Bridgman stops short just where, from the wider point of view of science as a whole, his ideas could be most valuable. It is true that every physical concept should correspond to a particular operation—indeed, to an operation of measurement, that is, an operation that yields a number, for physics is definable as the science of such operations. It is essentially an experimental science, where we pay attention only to observations made under narrowly prescribed conditions. But most of our experience is not so carefully prepared for; it comes to us automatically, whether we seek it or not, and the wider task of science is that of finding relations not merely between experiences that we go out of our way to obtain, but between the whole of our experiences. That opens up the larger field that, as I said at the beginning, I regard as belonging to the philosophy of physics.

I think it is one of the most satisfactory features of the modern view, expressed best in Bridgman's form, that it allows of the generalization we need. When physics is regarded as the study of the external world, and the world is regarded as a distribution of matter in time and space, there is little room left for anything else except illusions. But when the external world is forgotten, and physics is recognized as the finding of relations between certain artificially acquired experiences, the generalization becomes inevitable that science as a whole is the finding of relations between all elements of our experience. It is not that the question of the existence of the external world is considered and answered in the negative: the question is seen to be irrelevant and not considered. When I am asked whether I believe in the existence of the external world, I promise to reply when I am told precisely what is meant by the external world and by its existence. I have not yet been called upon to redeem the promise. The fundamental question is: What exactly is it that physicists are doing? That can be answered satis-

factorily only in terms of experience, not of the external world. It is in any event undeniable that all we can know of any world at all must come from our experience of it—not necessarily sense experience alone, but experience taken as a whole. If then we describe the task of science in terms of experience we shall describe it more directly than is possible in terms of an investigation of a postulated world. That might have been realized long ago ; the physics of the past century has made its realization inescapable.

THE 250TH ANNIVERSARY OF YALE UNIVERSITY

By Dr. JOHN F. FULTON, O.B.E.
Yale Medical Library

THE year 1951 marks the two hundred and fiftieth anniversary of the founding of Yale University, and under the leadership of its energetic new historian president, Mr. A. Whitney Griswold, the University is marking the occasion by a series of ceremonies.

The idea of establishing an institution of higher learning in New Haven was conceived many years before the College was actually established, having first appeared in the mind of the Rev. John Davenport, an Oxford man, who, with a group of his countrymen, founded New Haven in 1638. But New England was already supporting one College—Harvard—and it was not until 1701 that ten Congregational ministers met together, each bringing a gift of books, and made plans to petition the General Assembly of the Colony for a charter for a College, granted later that year.

Through the efforts of the Connecticut Colony's agent in London, Jeremiah Dummer, more books were soon on their way across the Atlantic from such well-wishers of the young College as Sir Isaac Newton, Sir Richard Steele, Dr. Edmund Halley, Dr. Richard Mead, Sir Hans Sloane, Dr. Richard Bentley, the King's Librarian, and many others. Among these books were a first edition of Newton's "Opticks" and a second edition of his "Principia", *Tatlers* and *Spectators*, the works of the Honourable Robert Boyle, Robert Hooke, Nehemiah Grew, Thomas Burnet, John Woodward, and such others as enable Yale proudly now to boast of the finest collection in America of books and manuscripts of eighteenth-century England—including the outstanding Falconer Madan Collection on the history of Oxford (acquired in 1929).

Our patron, Elihu Yale, was among these early friends of the College. The main facts of his life can be found on his tombstone in Wrexham :

> Under this tomb lyes interr'd Elihu Yale
> of Place-Gronow, Esqr ; born 5th April 1648,
> and dyed the 8th of July, 1721, aged 73 years.
>
> Born in America, in Europe bred,
> In Afric travell'd, and in Asia wed,
> Where long he liv'd and thriv'd ; at London dead.
> Much Good, some Ill he did : so hope all's even,
> And that his soul thro' Mercy's gone to Heav'n.
> You that survive and read, take care
> For this most certain Exit to prepare,
> For only the Actions of the Just,
> Smell sweet and blossom in the dust.

In addition to books, a gift of East India goods which brought £562 12*s*. in the Boston market came from Governor Yale and prompted the clergymen directing the destinies of the Collegiate School to change its name to Yale College in 1718 in honour of its generous benefactor. The portrait of King George I, which came with the other benefactions and which now hangs in the Yale Art Gallery, is to-day the earliest portrait in continuous ownership in an institution of higher learning in the United States.

In 1743 the Rev. Thomas Clap, the progressive president of the College who in his twenty-six years in office did much to broaden Yale's stature as an educational institution, "with considerable Labour and Pains, prepared a Catalogue of the Books in the Library under proper Heads so that you may Readily know and find any Book, upon any particular Subject". From this list of the books in the Library in 1742, a room has been arranged in the Sterling Memorial Library known as the 1742 Library, and on the shelves in their original order can be found many of the early volumes. Among the medical texts were works by Cowper, Willis, Freind, Cheyne, Sydenham, Sanctorius, Cheselden, Van Helmont, Harvey and Glisson—a noteworthy collection for any American library of that time. The Cowper was probably the gift of Daniel Turner, a London physician, who gave a number of books to Yale, for he wrote : "I have robbed my own library of a book very valuable beyond all others and now, as I know, not procurrable elsewhere, the Great Anatomy of our Cowper, that with it I might adorn yours". Turner's letter contained a postscript : "If your worships consider me worthy of the doctoral degree of Yale Academy and have the diploma sent me, I shall receive it not only as a sign of your gratitude, but I shall consider it an honour as much as though it had been conferred by another university, though of greater note. Farewell . . . and may your academy flourish." Rising to the occasion, the College authorities conferred on Daniel Turner in 1723 an honorary degree, the first medical degree to be granted in the American Colonies.

From Britain in these early days of Yale's existence came not only material gifts but also encouragement and inspiration for those who were interested in science, particularly in astronomy and mathematics. When in 1802 President Timothy Dwight persuaded the Corporation to establish a chair of chemistry and secured a young barrister, Benjamin Silliman, to fill it, it was to England and Scotland (after a year in Philadelphia) that Silliman journeyed to acquire his scientific knowledge. The contacts he made there at that time with many of the leading men of science, and later his *American Journal of Science*, to which British and Continental scientific men contributed, ensured a steady flow of influence and ideas from across the seas. His son-in-law, James Dwight Dana, who rose to eminence as a geologist, carried on in Silliman's tradition as editor of the journal and in the chair of geology.

Another outstanding Yale man of science well known in Britain and on the Continent was Josiah Willard Gibbs. He was a man of extraordinary capacity—shy and retiring as a personality, but possessed without doubt of one of the most gifted minds and one of the most original intellects of any nineteenth-century man of science. His genius was more promptly recognized in Britain than in America ; but we are happy to record that a definitive biography

of Gibbs has now been written by one of the last of his students, Lynde Phelps Wheeler, and published by the Yale University Press as one of the several volumes that have appeared on the occasion of the two hundred and fiftieth anniversary.

Yale's first gesture to mark the anniversary occurred at Commencement in June 1951, when twenty-five honorary degrees were conferred on outstanding American scholars (the usual number in a given year is eight or ten). One of these was conferred on a British subject, the distinguished Polynesian anthropologist and physician, Sir Peter Henry Buck, a native-born Maori chieftain who is now director of Yale's Bernice P. Bishop Museum in Honolulu. Another colourful degree-recipient was the ninety-two-year-old philosopher and educator, John Dewey, who was described as representing the "experimental spirit of America". The venerable Otto Loewi, Austrian pharmacologist and Nobel laureate, was also among those who received degrees, as was the psychologist Edward C. Tollman, "valiant defender of the freedom of the mind", lately removed from the faculty of the University of California for having, with many others, refused to sign the loyalty oath. Still another was Dr. Gerty Cori, the distinguished biochemist who in 1947 shared the Nobel Prize in Medicine and Physiology with her husband, Dr. Carl Cori.

The formal celebration of the anniversary will take place on October 19. To share in this ceremony the President and Fellows of the University have invited representatives from Oxford, Cambridge and Harvard, the universities from which Yale traces its descent, and in addition representatives from the forty-odd American colleges and universities which were originally administered by graduates of Yale. Prof. E. Ll. Woodward, professor of modern history in the University of Oxford, will be the official delegate from Oxford, and Mr. S. C. Roberts, lately vice-chancellor of the University of Cambridge, will represent Cambridge. There are to be formal addresses by Mr. A. Whitney Griswold as President, by Lord Halifax who will be the guest of the University, and by President James B. Conant, the official representative of Harvard University.

There will be a further occasion on Alumni Day, February 22, 1952, when President Griswold and the chairman of the Yale Alumni Board will award Yale Medals to alumni for outstanding service to the University. The anniversary year will come to an end with the Commencement exercises in June 1952. In the future, as during its first two hundred and fifty years, Yale will be mindful of the fundamental purpose of education, phrased by John Davenport as being "for the better trayning upp of youth in this towne, that, through God's blessing they may be fitted for publique service hereafter, either in church or commonweale".

OBITUARY

Prof. Gilbert Cook, F.R.S.

GILBERT COOK, who died on August 28, was born in Blackburn in October 1885 and he received his early education there and in Todmorden, Yorkshire. He distinguished himself at school, and in 1902 proceeded to Owens College, Manchester, where he studied under Osborne Reynolds. In 1905 he graduated in engineering with first-class honours and was awarded a graduate scholarship and the Freebairn Engineering Prize. After spending some time in the engineering workshops and as a pupil of the late D. C. Rattray, chief engineer to the Lancashire and Yorkshire Railway, he was appointed assistant engineer with that company. In 1910 he was awarded a Vulcan research fellowship at the University of Manchester, where J. E. Petavel was director of laboratories, and there he commenced the investigations which formed perhaps his chief scientific interest throughout his life. With Andrew Robertson he published in 1911 a paper on "The Strength of Thick Hollow Cylinders under Internal Pressure" and in 1913 one on "The Transition from the Elastic to the Plastic State in Mild Steel". They were followed by a number of authoritative publications on associated subjects spread over the years.

At the outbreak of war in 1914, Cook was an assistant lecturer in the Engineering Department of the University of Manchester, and he soon laid aside his university duties to serve first in the Royal Garrison Artillery and later in the technical branch of the mining and mine-sweeping service of the Royal Navy. Service routine did not smother his sense of scientific inquiry, and papers on the accuracy of range finders and on submarine depth meters were the outcome of his war-time experience.

Cook returned to his peaceful pursuits in the University of Manchester in 1919, and soon thereafter gained the award of a D.Sc. There followed in 1921 his appointment to the chair of mechanical engineering in King's College, London, where he enhanced his reputation as a teacher and administrator. It was in 1936 that he received the King's commission to be regius professor of civil engineering and mechanics in the University of Glasgow, and there he takes a worthy place in a succession which includes James Thomson and Macquorn Rankine.

Gilbert Cook was a member of the Council of the Institution of Civil Engineers and for many years a member of the Institution of Mechanical Engineers. The election to the fellowship of the Royal Society came to him in 1940. In Scotland he had given valued service to the Institution of Engineers and Shipbuilders, and he was just completing his period of office as president when he died. He served on many educational and technical committees, to which he gave conscientious service. He was consultant and advisor to the Admiralty Hydro-Ballistic Research Establishment, a member of the Advisory Council on Building Research, of the Board of Advisors in Engineering to the University of London and of the Properties and Mechanics of Materials Committee of the Department of Scientific and Industrial Research.

In the fifteen years of his tenure of the chair at Glasgow, Cook made a place for himself in the councils of the University and in the esteem of his colleagues which now cannot readily be filled. He was an excellent teacher, clear and convincing because he was a complete master of his subject. He was a sound administrator because he brought to the consideration of every issue with which he had to deal a logical mind and a conscientious judgment. He was a good colleague, frank, scrupulous, considerate and loyal. In addition to his great qualities of intellect and character, he had a cultured mind and an interest in music. He is survived by his wife.

NEWS and VIEWS

Imperial College of Tropical Agriculture: Prof. A. B. Killick, C.M.G.

The appointment of Mr. A. B. Killick to the Imperial College of Tropical Agriculture, Trinidad, as professor of agriculture, brings to the College a man whose wide experience in tropical agriculture will be of great value to its students. Mr. Killick was a postgraduate student at the College during the academic year 1923–24 and was appointed to the Department of Agriculture, Uganda, in 1924. He served in that Department until 1936, when he was appointed deputy director of agriculture, Trinidad. In 1939 he was promoted to the post of deputy director of agriculture, Tanganyika, and left there in 1942 to become deputy director of agriculture, Kenya. He was appointed director of agriculture, Uganda, in 1947, and is now retiring from that post. In 1950 he was awarded the C.M.G.

Mr. Killick's Colonial experience has covered a wide range of agricultural systems, from the primitive native tribal systems in Tanganyika to the partly mechanized European methods in the Kenya Highlands. In Uganda and Trinidad his work dealt with advanced peasant agriculture, which is approaching the freehold and leasehold farming methods, and he has also gained experience in plantation agriculture—sugar-cane, coffee, pyrethrum, sisal and cacao. He will, therefore, be able to give his students an overall picture of the type of agricultural problems which they will meet when they leave the College in Trinidad for Colonial territories, and they will also have the benefit of his experience in the economics and agriculture of the cotton industry in Uganda, which is now undergoing economic reorganization in view of its financial importance to the Commonwealth as a whole.

Physical Society: Parsons Memorial Lecture

The 1951 Parsons Memorial Lecture will be given under the auspices of the Physical Society by Mr. F. Twyman in the Anatomy Theatre of University College, Gower Street (not in the Science Museum as previously announced), on October 25, at 5.30 p.m. Mr. Twyman will take as his subject "Sir Charles Parsons and Optical Engineering". The Lecture was instituted in memory of Sir Charles Parsons, to be given in turn on the various subjects in which Sir Charles was interested. These included developments in optical instruments and engineering of all types, including naval. The lectures are thus arranged under the auspices of appropriate institutions, the Physical Society undertaking that concerned with optics. Sir Charles Parsons had a deep interest in the making of optical glass of fine quality, and Mr. Twyman has done much arduous and skilful work in improving methods of making high-quality glass and of testing glass and optical components. He was largely instrumental in the development of the Twyman–Green interferometer for the testing of optical flats and prisms for surface flatness and optical homogeneity, and he developed from it the camera-lens and microscope interferometer. In his book "Prism and Lens Making" are many instances of his ability to devise ingenious methods to improve the making and testing of optical parts. A further outstanding achievement was his making of echelon gratings of higher performance than those made by Michelson.

The Effigy of Robert Boyle

The Hon. Robert Boyle, the famous natural philosopher who was one of the founders of the Royal Society, was born at Lismore Castle, in the south of Ireland, on January 25, 1627, and died at his house in Pall Mall, London, on December 30, 1691. His sister, Lady Ranelagh, with whom he had lived, had died a week previously and they were both buried in the chancel of the old church of St. Martin-in-the-Fields, which was replaced in 1721 by the present building. In a letter in *Nature* of October 6, 1921, the late Sir William Tilden gave an account of his unsuccessful efforts to find any memorial to Boyle and concluded that "it seems probable that the last resting-place of the 'Father of Chemistry' will remain unknown to the end of time". Though this appears to be true, there is an effigy of Boyle as a very small boy in "the very famous, sumptuous, glorious tombe" of black marble and alabaster in St. Patrick's Cathedral, Dublin, erected by his father, the great Earl of Cork, in 1631 in memory of his second wife, who had died in 1629 when Robert Boyle was two years old. Richard Boyle, the first Earl of Cork, was born at Canterbury in 1566. After studying law, at the age of twenty-two he went to Ireland, where he held many important posts and became very wealthy. He was made an earl in 1620. His first wife having died in childbirth, in 1603 he married Katherine Fenton, the daughter of Sir Geoffrey Fenton (1530 ?–1608) and his wife Agnes Weston, and by her he had seven daughters and seven sons, of whom Robert was the youngest. The great monument, which now stands at the west end of St. Patrick's Cathedral, Dublin, has four tiers the highest of which has a recumbent figure of Robert Boyle's great-grandfather, Robert Weston (1515 ?–73), the Lord Chancellor of Ireland, and also dean of St. Patrick's. In the next tier are the kneeling figures of Boyle's grandfather and grandmother, Sir Geoffrey Fenton and his wife, while the third tier contains the recumbent figures of the Earl and Countess of Cork, with some of their children kneeling, and the lowest tier of all has representations of the seven younger members of the family, the effigy of Robert Boyle occupying a central arch in this stage. "It is a curious circumstance," says the official work on St. Patrick's, "that it is the only memorial to a man of science in the Cathedral." Though the great Earl is represented in the monument, he died at Youghal and is buried and commemorated in the parish church there. His death took place on September 15, 1643, when his famous son was but sixteen.

Memorial to John Milne

A movement is on foot to create a memorial to John Milne, who is commonly regarded as the 'father of seismology'. Space has been allotted in the Carisbrooke Museum, Isle of Wight, which is a mile from Shide, where Milne did the greater part of his life's work. Any seismological station having any discarded Milne or Milne–Shaw instrument, or parts, used prior to 1914 is invited to communicate with Mr. E. W. Pollard, Cliftonville, Dorking, Surrey.

Aeronautical Engineering Group of Aslib

Approximately a hundred representatives of the aeronautical industry in Great Britain attended a meeting held at the Royal Aircraft Establishment, Farnborough, on September 27 to inaugurate an Aeronautical Engineering Group of Aslib. This is

one of a series of specialist groups which are being formed among members of Aslib to consider the recording and dissemination of literature and technical information in special fields. The following committee was elected to control the work of the Group: Mr. R. G. Thorne, Royal Aircraft Establishment (*chairman*); Mr. C. W. Cleverdon, College of Aeronautics (*secretary*); Mr. A. H. Holloway, Technical Information Bureau, Ministry of Supply; Mr. T. G. Dickson, Rolls Royce, Ltd.; Mr. J. A. Kirk, Short Bros. and Harland, Ltd.; Mr. A. R. Cameron, A. V. Roe and Co., Ltd.; Mr. P. S. Walkins, British European Airways; Mr. F. H. Smith, Royal Aeronautical Society; Mrs. M. Neal, *Engineering*. 'Working parties' were set up to consider the following subjects: abstracting services; classification of aerodynamic data; union catalogue of the holdings of aeronautical libraries. Suggestions for consideration by the Group should be sent to the Secretary, Mr. C. W. Cleverdon, at the College of Aeronautics, Cranfield, Beds, or to members of the Committee. It is intended to hold an open meeting next spring to report on the work of the Group.

Meeting of Phycologists at Bangor

A GROUP of twenty-eight phycologists, the majority of them students of marine algæ, met at Bangor, North Wales, during September 13–20. During the week, excursions were made to the shores at Treaddur Bay, Rhoscolyn, Rhosneigr, Penmon Point and the Menai Straits, and one day was devoted to dredging in the Menai Straits, thanks to the kind co-operation of Dr. Crisp and the staff of the Marine Biological Station. These field meetings gave those attending the opportunity of comparing not only the composition of the flora with that of shores more intimately known to them but also the times of occurrence of reproduction of various common seaweeds. In addition, some lesser-known algæ were found and taxonomic difficulties discussed. Prof. P. W. Richards generously provided laboratory facilities, which were used to the full both for the examination of the freshly collected material and also for a series of valuable demonstrations, illustrating the very diverse types of research in progress. Although some of these had bearings on more than one type of problem, these exhibits could be classified broadly as ecological (Blackler, Moss), taxonomical (Burrows and Lodge, Parke, Powell), morphological (Martin, Richards, Mullahy, Dixon), and another group dealt with methods of culture and results obtained by this type of investigation (Parke, Knight Jones, Droop, Drew).

This informal group of phycologists has decided to hold further meetings, thereby hoping to encourage critical and experimental work on marine algæ, and anyone interested is asked to communicate with either Dr. K. M. Drew, Department of Cryptogamic Botany, University of Manchester, or Mr. H. T. Powell, of the Marine Biological Station, Keppel Pier, Millport. A duplicated news-sheet, containing notes of use to marine phycologists, will be available early in 1952, and the preparation of a check-list of the marine flora of the British Isles has been planned. A meeting of about a week's duration has been arranged provisionally to be held at Plymouth during July 1952 when, in addition to field meetings, consideration will be given to a special topic (possibly techniques used in the preparation of material for cytology and in the culture of algæ) and also to the taxonomy of a single family, probably the Delesseriaceæ. Inquiries about the possibility of a one-day meeting in London, during the first week in January 1952, are also being made.

Mythical Dwarf People in Polynesian Legend

THE existence of 'little people' is a belief that is of world-wide, though sporadic, occurrence. For the most part, they are dwarfish, more-or-less benevolent, hard-working in spite of their small stature, nocturnal and dwelling underground or in woods or forests. Such being their attributes, they are very difficult to pin down, as their habits make observation extremely difficult, and accounts of them vary with almost every supposed observer. In the Hawaiian Islands these nocturnal pygmies are known as 'Menehune', and Katherine Luomala, whose researches into Po'ynesian folk-lore are well known, gives an extensive and fascinating investigation of this subject in a booklet entitled "The Menehune of Polynesia and other Mythical Little People of Oceania" (Bernice P. Bishop Museum, Honolulu; Bull. 203; pp. 95; 1951). Her account of these tiny folk is gathered from many sources and includes their physical and cultural characteristics, their housing and social structure, their relations with other little peoples and their possible origin. All this gives them such a corporal existence that it is quite difficult to believe that they are mythical. They originate, apparently, from the old semi-slave common class of people who did all the hard work and were presumably stunted by toil and privation. The buildings they erected seemed worthy of a tradition, and so by the usual process of exaggerating the size of the subjects of the tradition both down and up (the clever little man and the stupid giant) these Menehune became smaller and smaller as time went on in order that their works might be more glorified. Other Polynesian islands also have their legends of Menehune or similar little peoples, and an account is given of the various myths connected with them.

Tarnish and Corrosion of Silver and Bronze

Two papers in the *Museums Journal* discuss the corrosion of silver and bronze respectively, problems which are of interest to a circle far wider than that of museum curators. The article on silver, by Dr. Bengt Thordeman and Mr. Gillis Olson, of the National Historical Museum, Stockholm, is contained in the February issue of the *Journal* and is a summary of their recent investigations of the well-known black or brownish tarnish of this metal. The present solution of the problem is to cover the cleaned silver with a film of nitrocellulose lacquer. The result of this treatment is not completely successful, for in some cases the protecting film gradually cracks and eventually gives rise to a network of unattractive black lines. The authors now consider that the rapidly developing plastics industry has provided a new method of approach, and a commercial lacquer of the acrylate type called 'Acryloid B7', produced by the Resinous Products and Chemical Co., Philadelphia, has been used. The silver is first cleaned by washing in dilute ammonia and removing the resulting film of black sulphide by contact with zinc or aluminium in a weak solution of sodium carbonate. After washing with distilled water and drying at 50–60° C., the still warm objects are dipped in an approximately 8 per cent solution of the lacquer. After excess of fluid has been drained off, the objects are baked at a temperature of 60–70° C. The other article, in the January issue of the *Journal*, is by Mr. J. F. S.

Jacks and is concerned with the cleaning and preservation of bronze statues. He reviews what occurs during the weathering of bronze, and considers the most effective methods to prevent corrosion and to remove the disfiguring products which result when corrosion has taken place. The ultimate colour varies according to the particular impurity present in the atmosphere. For example, oxygen and chlorine give a pale green chloride or oxychloride, while sulphur will produce a dark brown or black sulphide. Also, since impurities in the atmosphere and climatic conditions vary considerably at different times, the resulting patina is often uneven in colour, and a patchy effect is produced. At present it is considered that the application of a thin coating of lanolin forms the best protection from corrosion.

Australian Timbers and Veneers

The demand for plywood in Australia, as in other countries of the world, considerably exceeds the supply; extending production being accentuated by its wider use for housing, internal fitments, prefabrication and innumerable manufactured articles. In a paper entitled "Suitability of Australian Timbers for Veneers" (Bull. No. 260. Commonwealth Scientific and Industrial Research Organization, Melbourne, 1950), Messrs. A. J. Watson and H. G. Higgins summarize the results of investigations, extending over several years, into the suitability of Australian timber species for conversion into veneer, some fifty species having been examined, some systematically, others in a purely exploratory fashion. Certain of the species described are already in common use in plywood manufacture, others in limited quantities only and not for commercial purposes. It is these latter which should prove of importance, since the available supplies of the commonly used species is declining and it becomes necessary to look for new sources of veneer, both from indigenous forests and from plantations. In the case of the former, it has to be borne in mind that not only must the species be suitable, but also that it must be present in the (often mixed) indigenous forest in sufficiently accessible quantities to prove a commercially workable proposition. The data collected in the Division of Forest Products on the subject have been collated in tabular form. Information is given on some relevant characteristics of the trees and wood of the species examined, on their conversion into veneer from the log form, and on the recovery, characteristics and uses of the veneer. The general experimental methods of pre-treatment, conversion and drying are described in conjunction with certain recommendations to industrial operators, emphasis being laid on methods of determining suitable heating schedules for logs before peeling. Supplementary data are given on species recognized as sources of veneer, but which have not been investigated in the laboratory.

Forestry in British Guiana

From the forestry point of view, British Guiana has, perhaps, best been known for its greenheart; otherwise, although it has had a Forestry Department for nearly a quarter of a century, very little progress has been made, nor was much known about forests in the interior. The report of the Forest Department for the year 1949 (Georgetown, British Guiana) states that the previous year was a memorable one in the history of forestry in British Guiana, for it saw the announcement of the Colonial Development Corporation's forestry development proposals in the Colony, and the introduction for the first time of systematic management conditions into new and renewed timber leases. Complete reorganization of the Department was impossible owing to the difficulties in securing senior professional staff. For almost the first time on record in Colonial administration, the Governor himself gave a clear statement of forest policy proposed for the future. He remarked on the heavy capital expenditure needed for the construction of basic extraction facilities and the orderly exploitation of the forest in a series of contiguous blocks based on a predetermined programme. There must be complete control of felling operations in order to prevent undue waste and ensure retention of immature trees for future exploitation, and the retention of seed-bearers to provide natural regeneration; provision must also afterwards be made for improvement operations to ensure optimum growth of economic species. This certainly appears most promising, and an assurance has been given that the Forestry Department will not be overshadowed by the Colonial Development Corporation but that under its present conservator, Mr. C. Swabey, formerly well known in Trinidad, it will hold the reins of progress.

Fisheries Researches in Singapore Straits

One of the chief difficulties confronting fisheries development schemes in undeveloped and underdeveloped areas—which, incidentally, are mainly tropical—is ignorance. For the proper exploitation of marine fisheries resources, adequate knowledge of the bionomics of the sea areas supporting those fisheries is essential. Such knowledge, for most tropical regions, is extremely meagre or even nonexistent, and the appearance of a booklet, "The Food and Feeding Relationships of the Fishes of Singapore Straits", by Tham Ah Kow (Colonial Office Fishery Publication, 1, No. 1; pp. 35; London: H.M. Stationery Office, 1950; 3*s*. net), is therefore heartily to be welcomed. This publication embodies the results of an intensive study of the inshore fish fauna of Singapore Straits and of the environmental conditions in which it lives. No new tenets have emerged from this work, nor was any to be expected at this stage; but the application of old and well-tried techniques to a new area has produced useful preliminary knowledge of local conditions which can be amplified and supplemented by further researches of a more fundamental type. We look forward to the appearance of further Colonial Office fishery publications dealing similarly and progressively with this and other tropical regions.

Zoological Publications from Victoria University College, New Zealand

The appearance of the first six Zoology Publications from Victoria University College, Wellington, New Zealand, has already been noticed in these columns (see *Nature*, **166**, 1098; 1950), and four further papers are now to hand: No. 7, "A Study of the Marine Spiny Crayfish *Jasus lalandi* (Milne Edwards)", by C. A. Bradstock (pp. 38, 1950); No. 8, "Keys and Bibliography to the Collembola", by J. T. Salmon (pp. 82, 1951); and No. 9, "Blood Parasites of Mammals in New Zealand", together with No. 10, "A Contribution to the Study of Fijian Hæmatozoa", both by Dr. M. Laird (pp. 14+15, 1951). No. 7 is an account of this very widespread crustacean based on the examination of 2,126 specimens, and it deals with various aspects of their

life-history, ecology, population studies, fishery and utilization and includes observations on autotomy and autoplasy; a large female with a carapace-length of 13·5 cm. is estimated to produce 549,000 eggs. No. 8 is a paper that should prove of use to zoologists everywhere. It contains a bibliography which is complete from Delius and Linnæus (both 1758) up to and including 1949; it comprises 1,216 entries and is a valuable compilation. There is also a key for the whole of the Collembola carried down to genera; and, as the characters given are only those essential for generic identification, the name of each genus is followed by that of its author, the date and a number in brackets referring to the citation of that publication in the bibliography in which the original definition of the genus is to be found. This work is additionally welcome because it provides the first review of the entire order since the memoirs of Börner in 1906 and 1913.

British Contribution to Television: Convention in London

THE Radio Section of the Institution of Electrical Engineers is arranging a convention on "The British Contribution to Television", to be held in London at the Institution during April 28–May 3, 1952. The whole field of television will be covered in nine sessions, each approximately two hours in duration, at which a survey paper and a number of supporting papers will be discussed. First there will be a historical paper and a broad survey paper to act as an introduction to the whole convention, and this will be followed by sessions as follows: programme origination, point-to-point transmission, broadcasting stations, propagation, receiving equipment (two sessions), non-broadcasting applications, and system aspects. In addition to these technical sessions, there will be a number of visits to industrial and other appropriate organizations. Proofs of all papers should be available shortly before the meeting. The convention will be open to all; those who are not members of the Institution will be required to pay a registration fee. The proceedings of the convention will be published in four issues of Part IIIA of the *Proceedings of the Institution of Electrical Engineers*. Further details can be obtained from the Institution of Electrical Engineers, Savoy Place, London, W.C.2.

Colonial Service: Recent Appointments

THE following appointments in the Colonial Service have recently been announced: E. F. Allen and E. J. H. Berwick (agricultural officers, Federation of Malaya), senior agricultural officers, Federation of Malaya; H. Gillman (senior agricultural officer, Tanganyika), deputy director of agriculture, Nyasaland; R. W. Kettlewell (deputy director of agriculture, Nyasaland), director of agriculture, Nyasaland; A. B. Killick (director of agriculture, Uganda), professor of agriculture and director of studies, Imperial College of Tropical Agriculture, Trinidad; A. F. Mackenzie (senior agricultural officer, Sierra Leone), deputy director of agriculture, Sierra Leone; De W. N. Scott (agricultural officer, Federation of Malaya), senior agricultural officer, Federation of Malaya; D. Thornton (senior agricultural officer, Tanganyika), chief soil conservation officer, Tanganyika; A. C. Brooks (chief chemist, Federation of Malaya), director of chemistry, Federation of Malaya; I. A. Beveridge (senior assistant conservator of forests, Gold Coast), conservator of forests, Gold Coast; D. C. Duff (assistant conservator of forests, Gold Coast), senior assistant conservator of forests, Gold Coast; J. M. Cave (assistant agricultural officer, British Honduras), agricultural superintendent, St. Vincent, Windward Islands; C. A. Lea (assistant director, Meteorological Services, Federation of Malaya), director, Meteorological Services, Federation of Malaya; N. A. MacHattie (forester, Tanganyika), superintending forester, Tanganyika; R. H. Ball and P. Bradshaw, agricultural officers, Nigeria; D. V. Chambers, R. Frank, W. G. Mathewson and J. Russell, agricultural officers, Tanganyika; T. J. Forbes and K. Landskroner, agricultural officers, Gold Coast; R. N. Green, agricultural officer, Somaliland Protectorate; G. Heys, agricultural officer, Nyasaland; A. J. Jones, entomologist, Tanganyika; R. Knight, plant breeder, West African Cocoa Research Institute, Gold Coast; C. J. Piggott, soil chemist, Sierra Leone; P. B. N. Jackson, scientific officer, Northern Rhodesia; W. N. Scott, veterinary officer, Uganda; E. A. Thomas, veterinary officer, Tanganyika; R. M. Bredon, nutritional chemist (Veterinary Department), Uganda; J. O. Egerton, assistant conservator of forests, Federation of Malaya; J. G. Harris, assistant conservator of forests, Nigeria; G. M. Higgins, agricultural soil chemist, Nigeria; K. A. Hirst and A. B. Law, meteorologists, Northern Rhodesia; W. A. Sands, scientific officer, Termite Research, East Africa High Commission; Miss A. G. Smith, meteorological assistant, Bermuda; T. J. Tanner, meteorological officer, Northern Rhodesia; T. W. Tinsley, plant pathologist, West African Cocoa Research Institute, Nigeria; V. J. Weston, soil scientist, Nigeria; W. R. Williams, senior geologist, Tanganyika; R. A. M. Wilson, geologist, Cyprus.

Announcements

WE regret to announce the death on October 6 of Prof. O. F. Meyerhof, For.Mem.R.S., formerly director of the Kaiser Wilhelm Institute of Physiology, Heidelberg, and recently research professor of biochemistry, University of Pennsylvania.

H.M. THE KING OF DENMARK has bestowed upon Dr. D. A. Allan, director of the Royal Scottish Museum, Edinburgh, the decoration of Knight of the Order of the Dannebrog, in recognition of his work in furthering the knowledge of Danish culture in Great Britain and his co-operation with the Danish national museums and kindred institutions.

THE Council of the Illuminating Engineering Society has awarded the Leon Gaster Memorial Premium for 1951 to Mr. H. W. Cumming, of the Research Laboratories of Siemens Electric Lamps and Supplies, Ltd., Preston, for his paper entitled "The Extension of the Gas Arc Condition".

ON the recommendation of Sir Alfred Egerton, director of the Salters' Institute of Industrial Chemistry, the following awards have been made for the year 1951–52: *Salters' Fellowships:* Dr. A. P. C. Cumming and Raymond Grafton; *Salters' Scholarship:* Malcolm McLeman.

THE Sindicato Vertical de Industrias Químicas is holding a conference in Barcelona during November 8–15 on "Safety and Hygiene in Chemical Industry from the Point of View of the Manager, Technician and Operative". Further details can be obtained from the secretary-general of the conference, Sindicato Provincial de Industrias Químicas C.N.S., Vía Layetana, 18 Barcelona.

PHYSIOLOGICAL PROBLEMS OF MINING

ON August 9, at the recent meeting of the British Association in Edinburgh, Section I (Physiology) devoted a whole morning session to a number of papers and discussion on "Physiological Problems of Mining". Dr. G. C. Gooding (Scottish Coal Board, Edinburgh), in his review of the "Effects of Lighting on the Health and Working Efficiency of Miners", said that the development of adequate mine lighting in Great Britain has been hindered by the presence of firedamp. The evolution of the miner's lamp has been slow: the electric cap lamp was a big advance, and, although this is the commonest lamp in use to-day, in many districts other less-efficient lamps have not been replaced. Fixed lighting was first used at a shaft bottom in 1881 and is slowly extending. Installations of fluorescent lighting both at the shaft bottom and the coal face have been tried in the past five years and given promising results. Only a fraction of the light produced is reflected from the coal face and this impairs the illumination.

Miner's nystagmus is a disease, which may be incapacitating, associated with inadequate lighting. This disease is now much less common than it was thirty years ago, yet it has disabled for periods of one or more years more than fourteen thousand miners in the decade 1938–47. In some parts, Scotland for example, the disease appears to be checked though not abolished entirely. Continual caution is necessary, since the disease may exist in latent form, which may be converted to overt disability in adverse social circumstances.

With the best lighting available it is now possible to control miner's nystagmus; but good lighting is much more than prophylactic against this disease. Other benefits arising from improvements in lighting in mines are increased output and increased safety. Some small-scale trials have demonstrated both a reduction of accidents and the production of cleaner coal following improvements in lighting. The benefit which is probably the most important is an improvement in physical comfort and psychological temper. An improvement in lighting, sufficient to convert mines into underground factories, would work a revolution in the psychological climate in the industry. Increased comfort is a legitimate aim, being closely associated with industrial efficiency. The miner's job has been described as dirty, dangerous and dark. Attempts have been made, with at least partial success, to mitigate the dirt and reduce the danger. It is a legitimate aspiration for the industry to attempt to remove the dark, and with it another edge of asperity in the miner's life.

The next speaker, Dr. C. M. Fletcher (Medical Research Council Pneumoconiosis Unit, South Wales), discussed the "Effect of Dust Inhalation on the Working Capacity of Miners". Since statistics have been available, coal-miners (and especially elderly miners) have always been shown to suffer from a relatively high mortality from respiratory diseases other than tuberculosis and a relatively low mortality from pulmonary tuberculosis. This relative freedom from tuberculosis was ascribed by J. S. Haldane to a possible beneficial effect of coal-dust. Modern studies have confirmed that the inhalation of coal-dust modifies the effects of pulmonary tuberculosis and that in some ways this may be beneficial. Yet the serious effect of the inhalation of coal-dust on miners' working capacity has become increasingly apparent, especially as the result of admission of miners to compensation under the provision of the Workmen's Compensation Acts for silicosis, in the first place, and now for pneumoconiosis. The prevalence of pneumoconiosis has been very much greater in South Wales than in the other coal-fields, and the reason for this is not certain.

Pathological studies have shown that dust accumulates in and around the bronchioles where there is a minimal reticulin reaction to its presence. At this stage, simple pneumoconiosis, the dust could not be expected to cause any important disturbance of respiratory function. In elderly coal-workers the lung tissue in the neighbourhood of the deposit of coal-dust may become broken down to form 'focal emphysema'. If there are no complications, this focal emphysema is only occasionally so severe as to interfere with respiratory function. In some cases of coal-worker's pneumoconiosis, collagenous fibrosis occurs in the deposits of coal dust. Collagenous nodules of irregular shape form and later coalesce to form large areas of massive fibrosis which may seriously distort the lung and interfere seriously with its function, and in addition impose a strain on the pulmonary circulation.

Radiological studies enable a distinction to be made in life between simple pneumoconiosis and massive fibrosis, and a system of radiological classification has been developed. A correlation has been worked out between the radiological findings and a test of respiratory efficiency, and from this it is concluded that serious disability in coal-worker's pneumoconiosis is chiefly due to the presence of massive fibrosis.

"Coalminer's Pneumoconiosis: Employment Problems of the Disabled" was the subject of a paper by Dr. A. Meiklejohn (University of Glasgow). While hitherto the main incidence of certified pneumoconiosis has been in South Wales, there is evidence that, for various reasons, the number is increasing in other mining areas. It is not possible to provide satisfactory alternative jobs for these disabled miners. Reluctantly the conclusion has been reached that almost the entire responsibility for the continued employment of pneumoconiotic coal-miners must be accepted by and within the industry. The problem is not rehabilitation of disabled coal-miners, but rehabilitation of unfit coal-mines. In this assignment the main burden must fall on the employers and mining engineers, not on doctors and sociologists. There can be no speedy amelioration without the vital co-operation of the men themselves and their unions. This constitutes a challenge to discipline and leadership.

There are several important prerequisites which must be fulfilled. First, the whole problem must be extracted from 'politics'; secondly, it must be realized that pneumoconiosis of coal-miners is not peculiar to South Wales; thirdly, the fine focus on disease and compensation must give way to the acceptance that existent knowledge of the causation of the disease is already adequate, without further research, to enable effective prevention. Mining engineers in South Wales, by such methods as water infusion, have already made a useful start. The clamant demand is for more coal—higher and greater

production. To this end mechanization and more mechanization and more men to work the machine is the invariable answer. Is it quite certain that in some mines more coal-cutters and longer conveyor-belts are not indirectly diminishing output by the deleterious effects on the men? It has almost become trite among doctors to say this is an engineering problem. Until the disease is controlled so as not to abridge life or seriously impair health in a normal working span, then the medical man must bear his full part in the team. But in this particular field the medical man is simply an expert adviser; he is not the director in charge of operations. Though the production of dust must be kept to a minimum, considerable dust is unavoidable, and the occurrence and, in certain circumstances, the progression of the disease reflects the extent of the dangerous concentrations of air-borne dust. The medical man has two main duties: first, to assist the engineers to locate the dangerous operations and stations, and secondly, to supervise the health of affected men still at work.

The final speaker, Sir Andrew Bryan (National Coal Board, London), speaking on "Accidents in Coal Mines", said that of all the major industries in Great Britain, coal-mining is the most hazardous. Employing less than one-twentieth of the working population, it accounts for nearly one-quarter of all industrial accidents. The reasons for this high accident-rate lie in the nature of the work of the miner and in the vital differences between his working environment and that of other industrial workers. Mining is for the most part a continual striving against great natural hazards in confined and poorly illuminated spaces.

Up to the present time the attack upon the accident problem has been mainly by statutory regulation and inspection, assisted by scientific research into specific mining dangers and a raising of the standards of education and training of miners; and, more directly, by improvements in standards of housing, welfare and recreational facilities for the mining community as a whole. That these lines of attack have achieved a considerable measure of success over the years is shown by the progressive reduction in the accident-rates for fatal and serious non-fatal accidents. There is still much to be done along these lines. The attack must continue by way of further regulations as the results of new mining methods, machines and safety practices are devised; by way of higher standards of education or training for mine managers, officials and workmen; by the general adoption of higher standards of engineering production; and by the expansion of social and welfare activities in all mining communities. But new methods are needed if the progress of accident reduction is to be accelerated; and for this purpose new methods of investigation are required.

As examples of new lines of approach, the relationship between accident-rate, absenteeism and size of pit has been considered. There is a close correlation with the size of pit and the accident-rate per 100,000 man-shifts. In pits employing less than fifty miners the rate is 63·5, but rises to 166·6 in pits with more than three thousand. The reasons for this require study. Further, it can be shown that a high accident-rate can be correlated with a high pneumoconiosis-rate. Researches into observable features of human behaviour such as accident-rates, and their relation to the size of the groups in which men work, may, in the final result, throw light upon the structure of authority in the pit and the extent to which the men feel themselves to be part of the organization in which they are employed. Coal must always be got by small groups of men working in isolation and under remote supervision. The attitudes of these groups and the extent to which they each identify themselves with the pit as a whole are problems needing urgent investigation that confront the industry.

SENSES AND BEHAVIOUR OF ANIMALS

ON August 13 at Edinburgh the second joint session of Sections D (Zoology) and J (Psychology) of the British Association (the first was at Aberdeen in 1934) was held under the chairmanship of Dr. C. F. A. Pantin, the subject being the "Senses and Behaviour of Animals".

Prof. R. J. Pumphrey (University of Liverpool) spoke of the coming of age of 'ethology', a word recently applied to the study of the behaviour of all animals including man. Ethology, in this wide sense, evidently stems from the "Origin of Species" (1859) and the "Expression of the Emotions in Man and Animals" (1872); but its development has been arrested until very recently. This arrest is traceable to the influence of Descartes. In spite of the intellectual acceptance of evolution as a fact of history, a conscious or sub-conscious reservation in favour of dualism led to the persistent separation of a human ethology (psychology) which was rootless and arid, from an animal ethology, which carried the worship of the Golden Calf of objectivity to ludicrous lengths. The many 'languages of objective description' that have been applied to animal behaviour are seen in retrospect to cloak a belief in a physico-chemical etiology of behaviour, and an equation of meaning with statistical significance.

Prof. Pumphrey concluded that the time is now ripe for an integrated ethology with a common language and a common aim. A re-examination of the basic principles of semantics is needed, for semantics is doubly involved in ethology, both in the technique and in the description and interpretation of results.

The next speaker, Prof. J. E. Harris (University of Bristol), pointed out that a physiological and 'mechanistic' treatment is still appropriate to the genesis of behaviour patterns in the embryo. He showed in detail how this genesis can be related to the development of the muscular and nervous systems of young fish and amphibians, and how the first myogenic movements and rhythms are modified as motor and sensory nerve cells, and connexions and tracts become functional until the swimming rhythm is perfected.

Prof. R. W. Russell (University College, London) said that he was a psychologist and necessarily approached animal ethology with the terminology and point of view of human psychology. Nevertheless, he agreed with Prof. Pumphrey about the importance of semantics and the desirability of an integrated ethology with a common language. Such meetings as the present joint discussion and the Cambridge symposium on behaviour two years earlier reflect the desire of psychologists and animal ethologists alike to understand each other and to contribute jointly to an understanding of behaviour.

Prof. Russell then described the extensive field of animal psychopathology which has been explored

since the first studies in experimental neurosis in Pavlov's laboratory at the beginning of the century; and he convincingly demonstrated the close resemblance in the symptoms of neurotic behaviour of man and animals and in the situations which produce it. There is a fundamental difficulty in distinguishing 'normal' and 'abnormal' behaviour of animals in laboratory situations which are *ex hypothesi* abnormal. Nevertheless there is a promise, if not more than a promise, that animal and human psychopathology can be unified with significant results for our understanding of human behaviour.

In a review of learning by animals other than man, Dr. F. H. George (University of Bristol) emphasized the extent to which, after the earlier pioneering of Lloyd Morgan and Pavlov, the world was indebted to the United States for investigation in this field and for the elaboration of its somewhat indigestible theory. He emphasized in particular the unsatisfactory theoretical position of 'punishment', Muenziger *et al.* and Drew having shown that 'punishment' is as effective in reinforcing a right choice as in correcting a wrong one. He outlined the conflicting theories of learning of Tolman and Hull and seemed at least to hint that the conflict is partly verbal and terminological. Like previous speakers, Dr. George stressed the importance of a new semantic approach to ethological problems and suggested that a really comparative attitude is likely to be productive. In the 'comparative' psychology of recent years, 70 per cent of published papers deal only with the Norway rat.

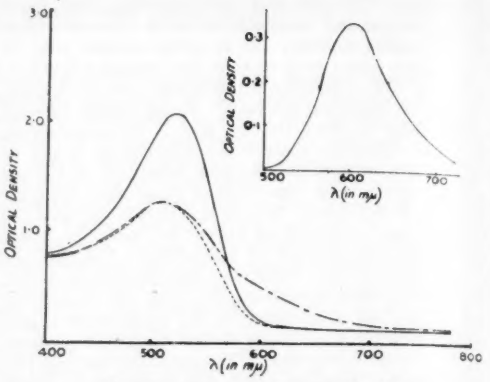
In the subsequent discussion, Prof. H. R. Hewer neatly pointed Dr. George's concluding remarks, and criticized by implication the abstractness and remoteness from reality of much modern work on learning and behaviour. During the Second World War he had been concerned with others in countering the depredations of wild rats. Search of the enormous literature on the behaviour of rats in laboratories had disclosed little that was relevant or useful in the control of wild rats. It was not at that time even known how they ate.

EFFECTS OF X-RAYS UPON PLASTICS

Electronic Processes

In continuation of our work on systems suitable for the dosimetry of ionizing radiations, and in particular the measurement of three-dimensional dose distributions[1], we have experimented with some coloured plastics. The observed phenomena, while indicating some applicability to the measurement of radiation, have aspects of more general interest.

It was found that of a series of thirty-two samples of commercially available polymethylmethacrylate ('Perspex'), coloured with various materials, three samples showed an appreciable change of colour when irradiated with X-rays. Similar results could be obtained using a plastic prepared by polymerization of an acrylic monomer coloured with benzene-azo-benzene-azo-β-naphthol (Sudan III). These colour changes are more intense than the slight colour changes shown by uncoloured 'Perspex', and of a different spectral composition. In addition, they are completely different from the slight bleaching caused in the same materials by ultra-violet light. The X-ray effect consisted in each case of the development of an absorption band at a wave-length



Absorption spectrum of 'Perspex Red 400'
Curve 1, —— before irradiation; curve 2, — · — immediately after irradiation; curve 3, - - - - 6 days after curve 2, in the presence of oxygen (complete penetration). Inset: Absorption band produced by the radiation (difference of curves 2 and 3)

longer than the absorption band of the basic dye, and manifested itself visually in a darkening of the colour. At the same time the optical density in the original absorption band of the dye decreased (see diagram).

The optical density in the new band produced by a given dose of radiation increases with the concentration of the dye in the plastic, and is also dependent on the concentration of oxygen. In the absence of oxygen the change in absorption coefficient is proportional to the dose (approximately 0·02 per cm. per 1,000 r. in 'Perspex Red 400'), whereas in the presence of dissolved oxygen in equilibrium with 1 atm. of the gas, scarcely any change occurs for the first 10^5 roentgens, and the rate of change increases at higher doses to approximately the same value as obtained in the absence of oxygen. If, after irradiation, oxygen is permitted to diffuse into the plastic, a partial reversal of the phenomenon occurs, so that the new absorption band due to the irradiation disappears; but the absorption at the original peak does not return to its normal value. A thermal back-reaction is also observed, which is very slow at room temperature, taking several months for completion.

These results are interpreted as showing that some of the electrons liberated from the lattice by radiation become trapped by and attached to the dye, the proportion depending on competition for the available electrons between the dye and other acceptors in the lattice. As a result of the irradiation, a system of positive holes and negative ions is formed. The phenomenon is analogous to the production of colour centres in alkali halide crystals. The system would not normally be chemically stable, but is evidently 'frozen' into the plastic lattice, and decomposes or recombines only very slowly at room temperature. This is a noteworthy difference from, for example, aqueous solutions, where the primary processes are presumably identical, but are followed much more readily by secondary chemical reactions.

Oxygen evidently competes for the electrons, forming O_2^-, and owing to its greater electron affinity is capable also of effecting an electron transfer from the dye:

$$\text{Dye}^- + O_2 \rightarrow (\text{dye} \ldots O_2^-),$$

where electron transfer has occurred, but the two species may be held together by the residual electron affinity of the dye.

The odd ions formed in these processes should exhibit paramagnetic resonance, and this has been found to be the case[2]. These coloured plastics are therefore solid systems, in which one has some control over the nature and number of the electron trapping sites. At the same time, the velocity of the thermal back-reaction is sufficiently small to permit accurate observation. The effect may be related to the increased conductivity[3] and fluorescence of plastics under irradiation[4] previously reported. It seems, therefore, that these systems might yield useful information on electron capture processes in solids. Further experiments on the chemical and physical aspects are in progress.

We wish to thank Imperial Chemical Industries, Ltd. (Plastics Division), for specimens of 'Perspex', Mr. W. A. Jennings for valuable comments, and Dr. F. T. Farmer and Dr. Joseph Weiss for their interest and support.

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May 26.

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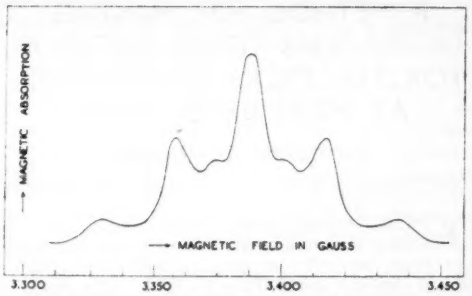
[1] Day, M. J., and Stein, G., *Nature*, **168**, 146 (1950); *Nucleonics*, **8**, No. 2, 34 (1951).
[2] Schneider, E. E., Day, M. J., and Stein, G. (see following communication).
[3] Farmer, F. T., *Brit. J. Radiol.*, **18**, 148 (1945).
[4] Wilson, C. W., *Nature*, **161**, 520 (1948). Winogradoff, N. N., *Nature*, **167**, 72, 123 (1951).

Paramagnetic Resonance

Using the sensitive micro-wave apparatus described recently[1], operating at 9,500 Mc./sec., sharp paramagnetic resonance spectra have been obtained with a number of plastics after irradiation in a 200-kV. X-ray therapy set. The main experiments have been carried out with specimens (2 cm. diameter × 0·3 cm.) of coloured polymethylmethacrylate ('Perspex')[2] and of clear uncoloured 'Perspex'. The observed intensities and widths of the resonance absorptions and their increase with X-ray dosage, which was varied between 10^5 and 10^7 roentgens, are in accordance with the assumption that the magnetic centres are electrons produced as the result of irradiation and trapped in the plastic lattice.

The structure of the resonance is the same for both types of specimen, consisting of a central peak symmetrically surrounded by a number of satellites (see graph). There is, however, a marked difference between the coloured and clear 'Perspex' in the position of the resonance spectra, providing evidence of differences in the nature of the trapping sites. In the coloured specimens the centre of the resonance lies at a magnetic field corresponding to a g-factor within 0·1 per cent of the free spin value, whereas in the clear 'Perspex' the resonance pattern is shifted down-field by some 20 gauss ($g = 2{\cdot}014$), indicating interaction of the trapped electrons with surrounding atoms. The symmetry of the resonance pattern of the dyed 'Perspex', in which no trace of the displaced clear 'Perspex' lines is detectable, suggests further that the presence of the dye molecules provides electron traps which are considerably deeper than those in the plastic host lattice. Other results point to the same conclusion, such as the stability of the



Magnetic resonance spectrum of irradiated 'Perspex'

trapped electrons in the dyed as compared with the undyed specimens shown by resonance experiments at long periods after irradiation, and the less severe competing effect of oxygen demonstrated by resonance observations at small X-ray doses with specimens of varying oxygen content. It should be noted that the electrons captured by oxygen molecules, and the positive holes, that is, the sites in the plastic lattice which have lost electrons during irradiation and consequently have unpaired electron spins, are assumed to produce no resonance effects detectable with the present apparatus. With this qualification the resonance experiments have gone far in supporting the arguments on the effects of X-rays on plastics presented in the preceding communication as the result of optical measurements.

The remarkable fine structure of the paramagnetic resonance of the observed resonance spectra is of even more general interest, since it appears to reflect characteristic properties of the structure of plastics. In the 'Perspex' resonances obtained at small X-ray dosage, lines are detectable at $\pm$ 12 and $\pm$ 46 gauss from the centre, besides the main satellites at $\pm$ 25 gauss. Resonances having a similar pattern have been observed with irradiated polystyrene; here the main satellites are at $\pm$ 18 gauss. It is believed that the satellite resonances are caused by regularities in the spacing in the plastic lattice of the trapping sites for the magnetic units (electrons and possibly positive holes). Magnetic dipole interaction between neighbouring spins would thus lead to a non-random distribution of the internal magnetic field acting on the resonating electron spins. Considering only the effect of nearest neighbours and neglecting the statistics of their spatial distribution, a crude estimate of a characteristic minimum separation r_0 between adjacent spins can be obtained by identifying the field separation of the main satellites from the resonance centre with the internal field μ_0/r_0^3 produced by a single adjacent spin. The values for r_0 of 17 A. and 19 A. derived from the observed resonance spectra for polymethylmethacrylate and polystyrene respectively may be related to one of the regular 'identity periods' derived for the specific plastics from X-ray diffraction measurements[3].

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May 26.

[1] Schneider, E. E., and England, T. S., *Physica*, **17**, 221 (1951); *Nature*, **166**, 437 (1950).
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A METHOD FOR MAKING ULTRA-THIN TISSUE SECTIONS FOR ELECTRON MICROSCOPY AT HIGH RESOLUTION

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DURING recent years, some techniques for making tissue sections for electron microscopy have been worked out which make it possible to prepare sections about 0·2 μ thick[1–7]. The resolving power of the electron microscope at this specimen thickness is estimated as 100–200 A., being different in different structures of the tissue cells because of differences in electron scattering power of the structures.

There seems to be a definite limitation of the techniques tried, due to compression and breaking of the sections when cut. In order to make use of the whole resolving power of modern electron microscopes, the thickness of the sections has to be only a few hundred angstroms or less. The demands regarding the thickness are determined by the size of the structural elements to be studied.

In order to obtain sufficiently thin sections for electron microscopy at high resolution, a technique has been worked out, the principle and first trials of which were published in 1943 [8–10]. The idea was to prepare sections without interference of the limiting factors mentioned above. This is possible by the use of a double sectioning technique. First of all, sections 0·5–1 μ thick of double-embedded (collodion–paraffin) material are cut on a Spencer microtome (No. 821) using the technique of Hillier and Gettner. Ribbons of serial sections are re-embedded in collodion on the accurately plane surface of a piece of glass. The surfaces of this glass are made to meet the same criterion for parallelness as are fulfilled by the well-known Johansson gauges, and in addition they are optically polished.

This glass 'measure' is placed in a specially constructed microtome on the plane upper surface of the advance mechanism, where it is kept in place simply through the cohesion between the plane surfaces in contact. A specially sharpened razor blade is used as knife, the edge of which travels parallel to the upper surface of the advance mechanism. The deviation from a parallel advance is 50–100 A. per mm. advance.

With this knife a greater part of the embedded sections is cut away and discarded, and the remaining part is mounted for examination in the electron microscope. This remaining part can be made extremely thin, as it has not been deformed during sectioning. The principle of cutting is illustrated in Fig. 1.

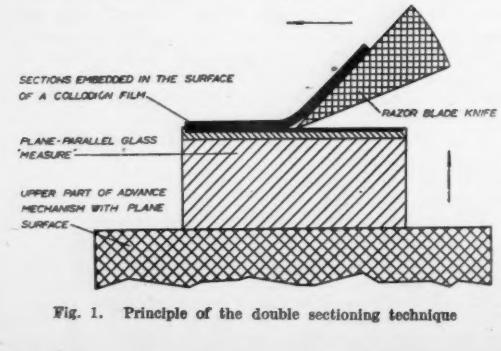
SECTIONS EMBEDDED IN THE SURFACE OF A COLLODION FILM
RAZOR BLADE KNIFE
PLANE-PARALLEL GLASS 'MEASURE'
UPPER PART OF ADVANCE MECHANISM WITH PLANE SURFACE

Fig. 1. Principle of the double sectioning technique

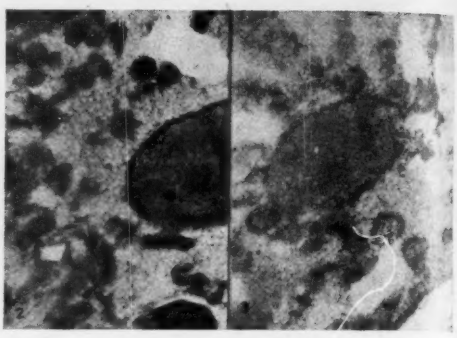

Fig. 2. Thicker region (about 0·2 μ thick) of section from mouse liver, fixed in 2 per cent osmic acid for 24 hr. 5,000 ×
Fig. 3. Adjacent thin region from the same section. In this picture the contrast has been much enhanced during photographic printing. 5,000 ×

In order to control the thickness of the sections, the thickness of the original sections has to be known and must not vary too much. The advance mechanism also must allow a very precise and accurate movement relative to the knife-edge. This movement is directly registered in angstrom units with an accuracy of better than 250 A.

The chances for getting sections of the right thickness are increased with experience of the processes of embedding and sectioning. From each ribbon of sections mounted on one glass 'measure' several sufficiently thin sections are obtained to allow the use of this technique as a routine method for the study of a large number of sections without an unreasonable expenditure of time.

For the analysis of the sections, an RCA *EMU* 2 *c* electron microscope with compensated standard objective pole-piece and the standard objective aperture was used. The pictures have been obtained with minimal electron bombardment. Fig. 2 shows a thick part of a section through a mouse liver. The thickness of this part of the section is estimated to be about 0·2 μ, judged from experience regarding the electron scattering of that kind of sections.

Fig. 3 shows an adjacent thin part of the same section, with the contrast highly accentuated during the photographic printing. The most characteristic feature of the thin sections is the very low scattering power, with a decrease of contrast differentiation of the larger structural elements of the cell when compared to the thick sections. On the other hand, the resolution is much improved and makes it possible to explore the ultra-structure of the cell organelles.

Fig. 4 shows a part of a section through the nerve myelin sheath from the sciatic nerve of a guinea pig at a magnification of 135,000. Periodic striations, the period of which is 50–80 A., are visible, presumably corresponding to a concentric arrangement of the membranes observed on fragmented nerve[11–12]. Indications of such a periodic structure in sections were first noticed by H. Fernandez-Moran[13].

The tissues were fixed in 2 per cent osmic acid (24 hr.). The conditions of fixation are critical when ultra-thin sections of this kind are prepared. The artefacts introduced through the fixation, embedding and sectioning will be systematically studied.

Fig. 4. Section through myelin sheath from guinea pig sciatic nerve, fixed in 2 per cent osmic acid for 24 hr. 135,000 ×

It is impossible to measure the thickness of these sections; but judging from the contrast of the sections as compared to the contrast of the supporting film when doubled at folds and ruptures, the thickness of the thinnest parts of the sections is estimated to be of the same order of magnitude as the supporting films ordinarily used, or a few hundred angstroms.

A more detailed description of the technique will be published elsewhere. [May 15.

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RESEARCH DEPARTMENT OF THE BRITISH BROADCASTING CORPORATION

NEW SITE AT BANSTEAD

IN the early days of broadcasting in Great Britain, the British Broadcasting Corporation soon found itself faced with a variety of technical problems which could not be resolved by the engineering maintenance and operating staff. A Research Department was formed in 1929, and five years later this section of the Corporation was housed at Nightingale Square, Balham. When, after the War, the normal work of the Department was resumed, it became clear that larger premises were necessary to deal with an enhanced programme of research. A suitable building, standing in twenty-seven acres of ground, was found at Kingswood Warren, Banstead, some twenty miles south of London. The mansion itself has been adapted to the needs of the Research Department staff with laboratories, offices and restaurant facilities. In addition, there is ample space for outdoor experimental work, as well as for the erection of two new laboratory blocks, one of which has been completed.

An 'open day' at this new site of the Corporation's Research Department was held on June 20, at which a number of exhibits and demonstrations were available for inspection to illustrate the work of the Department. On this occasion, also, an illustrated brochure, entitled "BBC Research Department, Engineering Division, 1951", was issued. This publication shows that the Department is organized into four Sections at Kingswood Warren, in addition to the Electro-Acoustics Group, which is still housed at Nightingale Square.

Among the most interesting activities at the present time is that of the Television Section, which deals with investigations into the electrical and optical performance of camera tubes as developed by the radio industry, and with the properties of the various circuits used in television technique. In addition, work is directed to an appraisal of the various problems involved in colour television, including its fundamental physiological aspects. The recording of television programmes on film is an item of investigation about to be undertaken.

The very limited number of frequency channels available for sound broadcasting has made it necessary for some time past for the Field-Strength Section of the Department to study the propagation of radio waves within the service area of each transmitting station. This work has recently been extended from medium frequencies to those involved in television and very-high-frequency sound broadcasting. Closely associated with this is the work of the Aerial and Transmitter Section, which is concerned with the development of aerial systems designed to give the largest local-service area of coverage or, in international broadcasting, to direct the programmes to the zone or country in which reception is desired. The Receiver and Measurements Section keeps abreast of developments in the design of broadcasting receivers and carries out tests on typical commercial products. It also designs receivers for special purposes, such as, for example, the standard receivers being used in tests on the relative merits of amplitude- and frequency-modulation in transmissions at very high frequencies.

At Nightingale Square, the Acoustics Section is responsible for experimental work connected with the design of studios and the basic principles of reverberation. Techniques have been developed in which the response of a studio to transient sounds is displayed on a cathode-ray oscillograph. A new method, not so far used elsewhere, makes use of a pulsed gliding tone, in which the frequency of the tone is gradually increased, and the changing form of the oscillograph display is photographed on slowly moving film. In the Recording Section, a thorough investigation of the disk-recording process has led to the design and construction of a disk-recording machine, since no equipment available commercially provides all the facilities required while meeting the standard of fidelity considered essential. Investigations in magnetic recording now absorb the major part of the Section's effort.

To assist in all this work, the Research Department has built up its own model shop, which is fully equipped to undertake the construction of the majority of the apparatus required; this is of a varied nature and may be of mechanical, electrical or optical type. As a normal part of its activities the Department maintains close touch with other broad-

casting authorities, with research organizations and with industry. Finally, great importance is attached to the publication of the results of research work; and the staff are encouraged to prepare papers for reading before scientific societies, or for publication in scientific and technical journals and in the *BBC Quarterly*.

NUFFIELD FOUNDATION

ANNUAL REPORT

THE outstanding feature of the sixth report of the Nuffield Foundation*, covering the year ended March 31, 1951, is the passage in which is considered the prospect of some curtailment of scientific research through rising costs, and whether the resources of such charitable trusts as the Nuffield Foundation should be used for sustaining academic activities thus endangered or for encouraging novel projects which may find it even more difficult to secure support. The present report indicates that, for the remaining three years of its present programme, the Foundation will continue to give preferential support to fundamental biological and social studies. This policy is succeeding, but its other main preference for the period—the encouragement of specialists in various fields to apply their knowledge and techniques to the study and solution of practical contemporary problems—has not attracted the same support. In addition, the Foundation continues to assist research of exceptional merit in any branch of science, and its schemes of training fellowships and travel grants are being expanded, and from the year's income of more than £550,000, some £55,250 was allocated for new or continuing schemes of this type, with a further £88,000 to be met from succeeding years. Of new grants totalling £336,194, £20,950 was used from the Oliver Bird Fund and other resources for research in rheumatism, £46,640 for fundamental biological and £111,000 for sociological research in the United Kingdom and £71,500 for research overseas within the Commonwealth. The original contribution of £500,000 to the National Corporation for the Care of Old People is not yet exhausted, and medical research on ageing is still similarly financed from an earlier allocation; no new grants have been necessary. As provision for a new headquarters when the Foundation is required to vacate its present premises, £135,000 has been set aside this year.

In continuation of its past programme the Foundation has increased by £3,650 the grant to the University of Bristol for Dr. C. R. Burch's work on the development of the reflecting microscope. Patents covering the present processes of manufacture offered to the Foundation are being put before the National Research Development Corporation for development in the national interest. In the medical sciences, the Foundation has made a grant of £750 for an additional full-time worker for epidemiological and bacteriological studies in the 'thousand-family' investigation being conducted by the Department of Child Health, University of Durham. In the social sciences, the Foundation is continuing its aid to Political and Economic Planning on the diminishing scale of £4,500 over the years 1951–53, while an additional grant of £1,000 has been made to the Caldecott Community to enable the children's reception centre to follow up the children who have passed through the centre or been 'placed' on its advice.

A gift of 1 kgm. of cortisone was made by Merck and Co., Inc., jointly to the Medical Research Council and the Foundation, and a new joint committee was set up to examine projects of clinical trial and research and allocate supplies both of cortisone and adrenotropic hormone. A three-year grant totalling £5,000 has been made to the Department of Human Ecology at the University of Cambridge for a medico-social survey of the Fen district of East Anglia covering the types and prevalence of rheumatic disease. A grant of £4,000 has been made to the Department of Chemistry, University of Birmingham, for the study of tendons, cartilage and tissue components and their structural chemistry, and £1,000 to the Medical School, University of Leeds, for special equipment for chromatographic techniques in the study of pathological collagen, while a grant of £1,050 a year for two years has been made for the appointment of a senior and a junior research fellow to work under Dr. D. M. R. Barton at Birkbeck College, London, on the synthesis of cortisone by the ergosterol route. The grant to the rheumatic unit at the Department of Medicine, University of Edinburgh, has been increased to £2,364, and that to Mr. A. Law, of the Orthopædic and Accident Department, London Hospital, for perfecting techniques of reconstructive surgery in chronic arthritis of the hips and spines, was renewed, as well as that to the South-West and Oxford Regional Research Unit, Bath.

New grants for fundamental research include £5,340 over five years, for a research assistant and technician under Prof. F. G. Young at the School of Biochemistry, University of Cambridge, for the perfection and running of the mass-spectrograph, and £1,000 a year for five years to the Physiological Laboratory at the same University for equipment and materials for workers under Prof. E. D. Adrian. A five-year grant totalling £20,000 has been made to Prof. C. Rimington, of the Department of Chemical Pathology, University College Hospital Medical School, London, for a small unit to continue and develop his investigations on the chemistry and biochemistry of hæm and other pyrrole pigments, and a grant of £500 for equipment and £400 a year for two years for a technical assistant has been made to Dr. E. Wangermann to enable her to continue at University College, Leicester, the work initiated under Prof. E. Ashby at Manchester on growth-rates of populations of common duckweed (*Lemna minor*) under controlled conditions. A further grant of £1,000 a year for five years has been made for an additional research worker to collaborate with Dr. C. Causey in work on certain aspects of nerve-cell body and axon as part of the work under Prof. J. Z. Young on the physiology of learning at the Department of Anatomy, University College, London, in which Mr. F. Roberts has now devised and built a 'flying-spot' microscope which has novel possibilities both for magnification and for counting and measuring particles of living tissues. A grant of £10,000 over the next five years has been made to the Department of Zoology and Comparative Anatomy, University of Oxford, for research under Dr. E. B. Ford on the evolutionary genetics of wild populations.

In the sociological field, the Foundation has offered a grant of £2,000 to the University of Birmingham to complete, under Prof. Charles Madge, the survey of a Midland market-town commenced in the Department of Social Science, and one of £4,000 to

* Nuffield Foundation. Report for the Year ending 31 March 1951. Pp. 113. (London: Nuffield Foundation, 1951.)

the Department of Social Studies, University of Leeds, for a two-year project to examine the changing relationships between the family and the community in an industrial village within easy reach of Leeds. The Foundation is also providing £3,750 for a survey by the Department of Social Science, University of Liverpool, of social relations in the dock-working community; under the general direction of Prof. T. S. Simey, this survey is intended to cover the relations between individuals and social groups which develop informally and spontaneously at work-level; between the subordinate and his immediate superior; and between individuals and social groups and officials and institutions with which they are brought into contact. An additional grant of £1,250 has been made to the London School of Economics and Political Science for the establishment of a Division of Research Techniques, and £100,000 over ten years for two additional fellowships, to be called Nuffield readerships, at Nuffield College.

New grants for research overseas include £900 a year for two years for a research fellowship for Dr. F. Douglas Stephens at the Children's Hospital, Melbourne, for research on problems of child surgery; up to £3,500 to the University of Melbourne, for appointment of a medical biochemist for two or three years in the Department of Biochemistry; £1,500 for special equipment for neuro-anatomical research in the Department of Anatomy, Histology and Embryology at the Christian Medical College, Vellore, and £300 a year for two years for a programme of meteor research at the Department of Physics, Canterbury University College, New Zealand.

The Foundation is also providing up to £12,000 for a review, jointly sponsored with the Secretary of State, of the policy and practice of education in the Colonial territories in Africa. The group investigating methods of work measurement used by industry in Great Britain is beginning inquiries as to why progressive managements continue to use time-study practitioners; what it is that management values, and why, in the existing methods; and what information industry would like to get from such techniques that existing methods do not supply. Fundamental scientific research into the measurement of human performance on skilled operations is also being initiated.

New fellowship and scholarship schemes during the year include a small number of biological scholarships and bursaries for graduates of the United Kingdom normally resident there, and sociological scholarships and bursaries for special training in sociological subjects, and a visiting studentship for a South African physiologist. Four United Kingdom medical fellowships were awarded; but there was even less response than in 1949 to the Foundation's offer of dental fellowships and scholarships. A final award of six fellowships, four postgraduate scholarships and ten vacation scholarships brought the total awards under the scheme for the advancement of extraction metallurgy to 23 fellowships, 21 postgraduate and 49 vacation scholarships. The scheme for travelling scholarships for young farmers is now extended to the whole United Kingdom. Only one travelling fellowship for the Home Civil Service was awarded in 1951; but India, Pakistan and Ceylon have now accepted the Foundation's offer of five, three and one fellowships respectively for study in Great Britain. Two further travelling scholarships were awarded to farmers of Southern Rhodesia for 1951.

For research into ageing, the Foundation has made a grant of £1,000 a year for two years to Dr. Alex. Comfort to work in the Department of Zoology, University of Birmingham, with a capital grant of £650 for apparatus and recurrent grants of £200 to the University for the costs of research. A grant of £2,600 over two years has been accepted by the University of Oxford for an examination, in the Nuffield Department of Surgery, of the changes in the blood-vessels of the brain as a result of ageing. An offer of £1,250 a year for a three-year trial period has been accepted by the Australian Committee of Vice-Chancellors for a full-time secretariat. A grant of £1,300 a year for three years has been made to the Scientific Department of the National Gallery to assist an examination, by the reaction to low-voltage soft X-rays, of the characteristics of organic materials, particularly synthetic resins, with the aim of discovering what can be learnt about condition and about picture structure as such, and also an examination of the swelling and solvent action of organic solvents on natural and synthetic varnishes and pigmented linoxyn films. A grant of £4,000 has been made to the Royal Institution towards the cost of reconstructing and re-equipping a modern workshop in the Davy Faraday Research Laboratory.

CHILDREN'S LEISURE

THE findings of a survey, carried out by the Social Survey Division of the Central Office of Information, into the out-of-school interests and activities of school children has recently been published*.

Information was obtained from the parents—chiefly mothers—of about 1,600 children, forming a representative sample of school-age children of England and Wales, and also from 840 older children themselves by means of a questionnaire which they filled in at school.

The survey was undertaken to assist the Central Advisory Council for Education (England), which, in 1947, was asked by the Ministry of Education to investigate the natural interests and pursuits of children out of school hours, the provision made for them, and the extent to which school work could help to develop these interests.

Thinking chiefly in terms of children's play, one in every two mothers (51 per cent) said that there was not enough for children to do in their districts. 36 per cent of the mothers were quite satisfied with existing facilities and 13 per cent gave qualified answers. Fewer mothers in rural districts (26 per cent) were satisfied than in urban districts (38 per cent).

The main cause of dissatisfaction was that the children had to play in the street; 47 per cent of the mothers said that their children usually played in the street. 46 per cent of the mothers said that there was no park available within easy reach of the children and 52 per cent said that there was no playground. As many as 52 per cent of the children in the sample did not go to any parks and 68 per cent did not go to a playground. Play-centres were almost unknown; only 2 per cent of the younger children (up to the age of ten) and 4 per cent of the older ones ever went to one.

The majority of the children lived within twelve minutes journey from school, so that, allowing for the usual school closing times, children appear to

* "Children out of School", by Joy C. Ward. 10s. net.

have approximately three to four hours in the evening, as well as the week-end, for their leisure interests.

But children have a good many calls on their leisure-time. Almost all (96 per cent) of the children at grammar schools had homework to do in the evening; 52 per cent of the boys and 39 per cent of the girls did more than one hour per evening. Only one in four of the children at secondary modern and technical schools did any homework, and the majority did less than one hour. Grammar school children—particularly the girls (42 per cent)—had other studies as well, music practice taking up most time.

Apart from homework, both boys and girls had considerable duties at home, the girls more than the boys. Shopping headed the list of duties: 80 per cent of the girls and 68 per cent of the boys did some shopping. Helping at home came next: 78 per cent of the girls and 59 per cent of the boys had some duties at home, which ranged from small tasks like setting meals to being responsible for the family's evening meal. 43 per cent of the girls and 32 per cent of the boys looked after younger children in the family.

Investigating membership of clubs and organizations among the children, Miss Ward reported that children are 'club-minded' by the age of ten, although most youth clubs do not open their doors to children below their 'teens'.

Three-quarters of the children of 11–15 years belonged to one or more club or other organization. Cinema clubs were mentioned more than any other and were most popular at the age of 10–11 years; 22 per cent of the boys and 18 per cent of the girls belonged to them. 13 per cent of the boys were Scouts or Cubs and 16 per cent of the girls were Guides or Brownies. The highest proportion of membership was among the 11–12 year olds.

From the age of thirteen upwards social clubs of various kinds were the most popular clubs and increasingly so with age. Church social clubs began to be important in the 8–10 year age-group (boys 11 per cent, girls 10 per cent). Other social clubs did not show comparable membership until the age of thirteen. Only 1 per cent of children in rural areas belonged to youth clubs compared with 7 per cent in towns.

A small proportion of the 11–15 year old children belonged to clubs which were devoted entirely to sports. More boys (10 per cent) than girls (3 per cent) belonged to these.

Cinema-going was the largest single activity among the children: only one in five did not go at all and those that did not go were mostly the very young ones. Only 6 per cent of the children of eleven and over did not go and less than 1 per cent of the 14-year olds. Most frequent was the weekly visit to the cinema, but 30 per cent of the older boys (11–15 years) and 25 per cent of the older girls went more than once a week. Grammar school children went less than those at secondary modern and technical schools. It seemed that boys and girls went equally as often, though there was evidence to suggest that girls at grammar schools went less than the rest of the children. Boys in the lower economic groups went more frequently than those in the middle and higher groups.

Membership of cinema clubs was found to be strongly associated with frequent visits to the cinema; compared with other children nearly twice as many of the children who belonged to cinema clubs went to the cinema once a week and more than once a week. The films the children (11–15 years) liked best were 'thrillers', murder and mystery films. The girls' second choice was 'musicals', and the boys' second choice was comedies.

Only 6 per cent of the 11–15 year old children said that they did not listen to the radio; one in three listened on the evening prior to the interview with the investigator. Among the younger children 14 per cent of the 5–7 year olds and 8 per cent of the 8–10 year olds did not listen in. Most of the older children (11–15) had hobbies of some sort; only 9 per cent of the boys and 12 per cent of the girls did not mention any.

Reading was more popular with girls than with boys, at all ages. Only 8 per cent of the 11–15 year old children said that they did not read. 28 per cent of the children in the sample belonged to the public libraries.

Football and other games were by far the most popular activities of the boys. 77 per cent mentioned it as one of the four things they liked doing best out of school. Next to this, playing out of doors, walking, and cycling were mentioned by 52 per cent of the boys, and (heading the girls' list) by 59 per cent of the girls. Other interests were considerably less popular with boys, but the girls' interests, lacking a predominating factor such as football, were more evenly divided. Besides playing out of doors, girls liked reading (53 per cent) and indoor activities such as knitting, sewing and playing 'indoors'. The girls' interests were found to lie much more at home than the boys'. Seven out of ten of the boys sought their leisure-time interests away from home compared with about one in three of the girls.

The investigator also made detailed analyses into the interests and activities of children at grammar schools, on one hand, and those at secondary modern and technical schools on the other. Apart from better facilities for play—such as more parks in their own district—the grammar school children's time was more fully occupied. They had, on an average, a slightly longer journey from school, and the majority did more than an hour's homework each night. Many of the girls, in addition, had music practice and other lessons to do. On the other hand, as many grammar school girls helped at home as secondary modern and technical school girls, although more of the latter had to look after younger children in the family. More grammar school girls belonged to the Girl Guide movement, and more played games. Wider interests of grammar school girls were shown also in the fact that membership of such societies as the Young World League and Dr. Barnardo's League was confined almost entirely to them. Membership of hobbies clubs was confined almost entirely to grammar school children; 30 per cent belonged, compared with 12 per cent of the children at secondary modern and technical schools.

Children at secondary modern and technical schools went to the cinema more often than grammar school children, and twice as many of them belonged to cinema clubs. More of the boys and girls at secondary modern and technical schools belonged to no other clubs apart from the cinema clubs.

Some differences were found also in the kind of films and radio programmes which the children liked, though the favourite radio programmes—'Quiz' and 'Variety'—were liked equally by girls and boys at both types of schools.

Since this survey was carried out in the winter months, it is possible, as Miss Ward suggests, that different findings would be made if investigations were continued in a summer period.

LETTERS TO THE EDITORS

The Editors do not hold themselves responsible for opinions expressed by their correspondents. No notice is taken of anonymous communications

Electron Microscopy of Chick Embryo Membrane infected with *PR*-8 Influenza

PREVIOUS studies[1] of tissue sections with the electron microscope have shown masses of particles having the dimensions of the elementary bodies of influenza virus associated with infected cells. These particles have been the expected mixture of spheres and rods with diameters of *c.* 100 mμ. As seen in the sections, they have seemed to be developing out from, rather than within, cells. This manner of growth and the ease with which it can be observed make influenza-diseased tissues one of the more fruitful objects for the study of how viruses grow.

In the earlier investigation, the virus-like particles were observed both on the chorio-allantoic membrane of chick embryos and in the lungs of mice. They have, however, been found more easily on membranes. A striking feature of infections with freshly isolated strains of virus has been the rapid necrosis that has followed infection, and the difficulty of finding virus particles in the slimy masses of disintegrating tissue that result. For this reason it has seemed worth while, as a next step towards seeing how influenza virus proliferates, to examine the infection of chick embryo membranes using a long-established and well-adapted strain of virus such as *PR*-8.

For these experiments the membranes from inoculated embryos were harvested after 48–72 hr. incubation, washed with saline and fixed by immersion for one hour in neutralized 4 per cent formalin–saline. Pieces were dehydrated by passage through pyridine–water mixtures and then embedded in methacrylate[2] polymerized in an oven at 45° C. Sections cut from such blocks were mounted on 'Formvar'-covered grids, the methacrylate was removed with amyl acetate and the resulting preparation lightly shadowed with palladium for examination under an RCA type *EMU* microscope.

Masses of filaments and spheres having the diameter of virus are to be seen at many points along surfaces of these membranes (Fig. 1). Many of the filaments appear segmented into the spheres. Filaments develop both out from the limiting surface of the membrane (Fig. 2) and between cells. As this latter growth proceeds, it isolates adjacent cells more and more completely from one another; a cell thus desquamated is shown in Fig. 3.

It is obviously of primary importance to try to establish whether the filaments are protoplasmic protrusions from the affected cells or parasitic growths along its borders. It has been difficult to find convincing evidence in the photographs for one or the other of these possibilities; but it is noteworthy that where sectioning has cut through attached filaments, their contents seem continuous with the cellular cytoplasm (Fig. 2). To this degree the filaments and spheres shown in the figures can be thought of as extruded and detached bits of this cytoplasm.

The question naturally arises as to whether these objects actually are the virus particles. After formalin-fixation, they invariably have had the diameters (*c.* 100 mμ) of the infectious units found in purified suspensions. Large areas of infected tissue were scanned during the present experiments; the particles illustrated could not be found in all membranes, but no other objects the size of virus were anywhere recognized. Healthy membranes have not contained them, but similar particles have been numerous in sections through membranes infected with Newcastle disease virus[3]. In two or three out of several hundred membranes, filaments of the diameter in question have been seen along the borders of membranes that were not diseased with either of these viruses. It is not yet known what were the conditions that produced them.

Using dark-field optical microscopy, Hoyle has seen indications in pieces of excised normal and influenza-diseased chorio-allantoic membrane[4] of what may be the cellular protuberances shown here. It is hard to make too close a comparison between the dark-field observations and the electron micrographs, because particles the size of influenza virus are so small that optical microscopy cannot portray them in their true dimensions even though it may indicate their presence. Nevertheless, the electron microscopic observations thus far made suggest a picture of

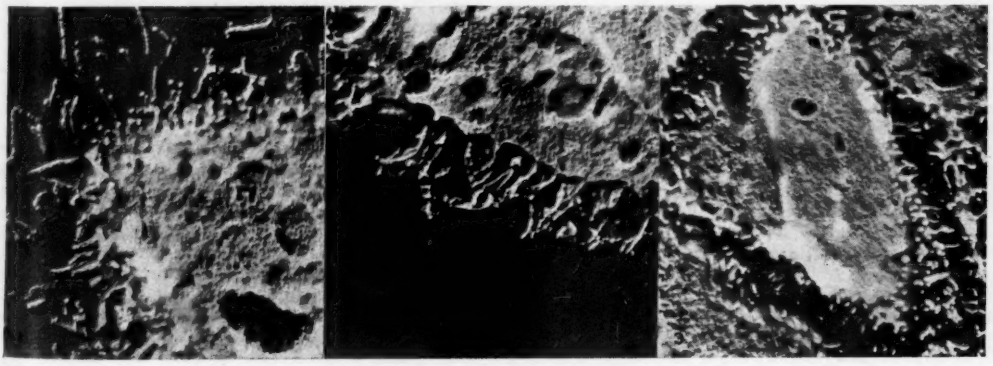

Fig. 1. Rods and spheres the diameter of influenza virus along the border of a diseased cell. A few of the rods seem to be segmenting into the spheres. Note the spongy texture of the filaments. × 10,000

Fig. 2. Filaments developing out from the periphery of a *PR*-8 diseased cell. Where the filaments, too, have been sectioned their contents seem continuous with the cytoplasm of the cell. × 10,000

Fig. 3. Remains of a cell isolated from its neighbours by the filaments that have developed around its borders. × 6,500

developing influenza virus which resembles in many respects that proposed by Hoyle. If the elementary infectious units of influenza actually are cellular fragments, as the electron micrographs indicate they may be, this defines within narrowing limits the views that can be maintained concerning the fundamental nature of influenza virus. The study is being continued.

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Aug. 3.

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Action of Living Cells on Cortisone Acetate Crystals

In the course of experiments designed to study the effect of cortisone upon the growth of fibroblasts in hanging-drop tissue culture, it was observed that the cortisone acetate crystals dissolved in the plasma clot.

One drop of fowl plasma and one drop of embryonic extract, both diluted with Gey's saline solution in the proportion of 1 : 2, were mixed with one drop of cortisone acetate (Merck) suspension. This mixture of the three drops was spread out on a cover slip, and a piece of chick embryo tissue 1 mm. in diameter was placed in the centre of it. The cover slip was then affixed to a depression slide, sealed and incubated at 38° C.

Fig. 1

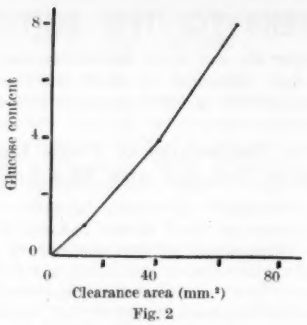

Fig. 2

When a higher concentration of cortisone acetate is used, the plasma clot appears macroscopically turbid, and under the microscope the crystalline form of cortisone acetate may be seen discretely suspended in the otherwise clear plasma clot. With lower concentrations of cortisone it was found that the crystals disappeared after 24 hr. incubation; but in the higher concentrations, only a circular zone around the tissue was cleared of crystals. It was further observed that there was extensive vacuolation of the growing cells which, according to Fisher (1947), may be a sign of depletion of glucose. As a result of this observation, and in view of the fact that a related compound, deoxycorticosterone, is known to be soluble as a glucoside, further series of tissue cultures with increasing amounts of glucose were set up.

The cortisone acetate (Merck) used in these experiments contains 25 mgm./ml. and was diluted with Gey's saline solution containing 0·2 per cent glucose. Cortisone concentrations of 10 per cent (*A*), 20 per cent (*B*) and 40 per cent (*C*) were used. The size of the area cleared of cortisone crystals around the implant up to 24 hr. is recorded in Fig. 1. The series containing 10 per cent cortisone (concentration *A*) cleared rapidly, and after about 12 hr. all the cortisone crystals had dissolved in the plasma clot.

With 20 per cent cortisone (concentration *B*), during the first 24 hr. up to an area approximately 9 mm. in diameter around the implant was cleared and did not change on further incubation. With 40 per cent cortisone (concentration *C*) the area cleared was limited to 5 mm. in diameter.

After 5 hr. incubation, some preparations of each series were taken out of the incubator and further observed at room temperature. As is shown in Fig. 1, the process of clearing was slowed down considerably at the lower temperature. The reason for this may be the slower metabolism of the living tissue at sub-optimal temperature.

In order to observe the effects of increased glucose content, another series of hanging-drop cultures was set up, each containing the 40 per cent cortisone suspension, but the Gey's solution used to dilute the cortisone contained 0·2, 0·8 and 1·6 per cent glucose.

It was found that the higher the glucose content, the greater was the area cleared around the implant. Fig. 2 shows this to be a nearly linear relationship.

When dead tissue instead of living tissue was embedded, the above-described phenomenon did not take place. Cholesterol crystal suspensions used instead of cortisone were not dissolved by the living tissue culture.

These observations indicate that the dissolution of the cortisone acetate crystals is dependent upon chemical changes associated with living tissue, and

the fact that glucose is essential to, and promotes, the process suggests that the resultant compound may be cortisone glucoside.

The cortisone used in this experiment was part of a gift kindly made by Merck Co. (Inc.) to the Medical Research Council and the Nuffield Foundation.

Detailed experimental results will be published elsewhere in due course.

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The 1·5-A. Reflexion from Proteins and Polypeptides

In a recent issue of *Nature*, Pauling and Corey[1] have raised certain points in connexion with the 1·5-A. reflexion which I reported to be present in proteins of the α-keratin type, in an artificial polypeptide and in hæmoglobin, and to be absent in feather rachis[2]. To their remarks I should like to make the following comments.

Occurrence and significance of the reflexion. In their most interesting and stimulating paper, Pauling, Corey and Branson[3] have given powerful arguments to show that, of all the possible models of polypeptide chain, their newly discovered 3·7- and 5·1-residue helices have the lowest free energy, from which they conclude that these two configurations are the ones most likely to occur in Nature. They have also reported that one of these chains, the 3·7-residue helix, gave favourable agreement with published X-ray data of fibrous proteins and artificial polypeptides of the α-keratin type, and of hæmoglobin. Pauling, Corey and Branson's solution of the stereochemical problem seemed convincing; on the other hand, Bragg, Kendrew and I[4], and others before us, had found that some of the alternative models also fitted X-ray data reasonably well, and it seemed to us that such correlation as Pauling and Corey had obtained with the 3·7-residue helices did not rigorously exclude those other models.

These doubts led me to predict that the 3·7-residue helix, if present in all proteins and polypeptides of the α-keratin type, should give rise to a reflexion at 1·5-A. spacing when the fibre axis is inclined at equal angles to the incident and diffracted rays. None of the models previously proposed would give a reflexion at this spacing. So far my prediction has been borne out by experiment. In addition to the substances for which the reflexion was first reported, Huxley and I have now found it in wool, myosin and actomyosin, Astbury in epidermin[5] and Brown in poly-γ-methyl-L-glutamate[6]. It is interesting that while the exact spacings of all other α-keratin reflexions vary slightly in different substances, this reflexion appears always at the same spacing of 1·50(± 0·01) A. This suggests constancy of the configuration of the main polypeptide chain, no matter what the side-chains are.

MacArthur, in the course of his detailed X-ray studies of fibrous proteins, discovered a reflexion at 1·49 A. in African porcupine quill, but not in Canadian porcupine quill, nor in any other fibre[7]. I did not regard the appearance of this reflexion in African porcupine quill alone as decisive evidence, because this fibre is now known to contain a substantial fraction of a protein of the β-type[8], to which this reflexion might possibly have been due, and also because MacArthur[7] and Astbury[9] had interpreted the meridional reflexions of porcupine quill in terms of the repeat of different kinds of amino-acids at regular intervals along the chain. I selected polybenzylglutamate as a test case, first because Ambrose and Elliott[10] had shown it to contain predominantly chains of the α-type, secondly because diffraction effects due to the regular repeat of different kinds of amino-acids can be excluded in a simple polymer, and thirdly because the intensity of the reflexion should be high in a substance where all the side-chains are of the same kind. This expectation was confirmed by results.

The points brought out in the communications by Huxley and myself in *Nature*, which I believe to be new and important, are the universal appearance of this high-angle reflexion, in an artificial polypeptide, in such poorly organized fibres as hair and muscle, and in a crystalline protein; its remarkably constant spacing; its disappearance in the β-form; and its decisive character, not in proving the 3·7-residue helix to be correct, but in excluding models which do not have a repeat of 1·5 A. or a multiple of that spacing. I think it is fair to claim that none of these points had been previously realized. Had the existence of this high-angle reflexion in all fibres of the α-keratin type been discovered earlier, there is little doubt that it would have excluded many models of chain which have been put forward and probably led directly to a solution of the type at which Pauling, Corey and Branson arrived largely by stereochemical reasoning.

Reflexions at multiples of 1·5 A. Pauling and Corey[11], and in the statement quoted in their later communication[1], say: "We would expect that side-chains of different kinds on the α helix would repeat at an integral number of residues, corresponding to an integral multiple of residue lengths along the helix axis, about 1·53 A., and that accordingly those orders of basal plane reflexions for which the spacing approximated closely to certain multiples of 1·53 A. would be enhanced in intensity". This would be a quite different feature of the chain from that indicated by the universal and constant 1·50-A. spacing, and one which still appears to require proof. It would require the repeat of the same or similar amino-acid residues at regular intervals, for example, every other or third residue having a large side chain. The results of partial hydrolysis of proteins, however, do not indicate that such regularities are a feature of protein structure.

Huxley and I have therefore re-investigated the appearance of these reflexions, and our results to date, combined with those of previous investigators, have led us to the following conclusions. (*a*) In polybenzylglutamate and in homogeneous proteins of the α-keratin type, such as myosin and fibrinogen[12], these reflexions are absent. They are also absent in our specimen of porcupine quill, although this does show the 1·5-A. reflexion itself. (*b*) Systems such as hair and porcupine quill which Ambrose and Elliott[8] have recently shown to contain mixtures of α- and β-proteins do show reflexions at spacings which are approximately, but not exactly, small multiples of 1·50 A. Horse hair shows a wide meridional arc at 3·08 A. which is almost certainly due to β-keratin, as its spacing is the same as that of a strong meridional reflexion in feather rachis. The reflexions at 3·08 and 6·19 A. which MacArthur observed in

African porcupine quill both correspond to strong reflexions in feather rachis and may have the same origin. (c) Astbury[13] and MacArthur[7] have shown that reflexions from frog muscle at approximate multiples of 1·50 A. are due in part to collagen and in part to actin. Bear's clam muscle (type II) gives reflexions similar to those of frog muscle which may be accounted for in the same way[14]. In clam muscle (type I) Bear does not list any meridional reflexions below 18·2 A. (apart from the 5·1-A. reflexion)[15]. When such high multiples of 1·50 A. are considered, spacings would have to be known with great accuracy before a correlation could be definitely established.

Feather rachis keratin. I agree with Pauling and Corey that the absence of the 1·5-A. reflexion does not necessarily exclude the presence of 3·7-residue helices. Their remarks have led me to re-examine the X-ray diffraction pattern of feather rachis. Oscillation photographs taken about the normal to the fibre axis indicate that the strong reflexion at 3·07 A. has a structure factor which is about twice as large as that of any other meridional or near-meridional reflexion. The reflexion at 5·53 A., quoted by Pauling and Corey in support of the presence of 3·7-residue helices, is weak and consists of two separate reflexions the normals of which make an angle of about 22° with the meridian. There is no reflexion at 0·75 A., such as would be expected on the basis of the structure proposed by Pauling and Corey[1]. These results suggest that the correct solution may be of the type originally proposed by Astbury and Woods, with chains of the β-type playing a dominant part in the crystalline fraction of the fibre[16].

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Action of Terramycin and Chloromycetin on Cholera Vibrio in Mice

Terramycin hydrochloride (Pfizer) was tested in mice inoculated with *Vibrio cholera*. The animals were inoculated intraperitoneally with a suspension of 1 ml. of an 18-hr. old culture; the suspension contained 50×10^6 organisms per ml. and 5 per cent mucin (Armour)[1]. These inoculations were fatal in 48 hr. Four strains (Ogawa 41, Inaba 35 and 29, and a Mediterranean Inaba strain) were employed.

In the first series of experiments, in which the antibiotic was administered intraperitoneally in single graduated doses half an hour after the infection, 15 out of 15 animals, infected with the Inaba strains, recovered after treatment with 5–10 mgm./kgm. of terramycin. The strain Ogawa 41 was more resistant and required 15 mgm./kgm. in order to produce a 100 per cent survival.

Larger doses of antibiotic were required to control the infection when administered five hours after inoculation: 15 mgm./kgm. in the case of the Inaba 35 strain, 30 mgm./kgm. for the Inaba 29, the Mediterranean Inaba, and the Ogawa 41 strains. With these quantities, all 15 animals of the series survived.

A period of five hours seems to be the limit of time after infection in which the terramycin is effective. In experiments with the Inaba 35 strain, even 300 mgm./kgm. was ineffective after seven hours from the time of infection.

If administered half an hour after the infection, chloromycetin (Parke, Davis and Co.) was curative in doses of 25 mgm./kgm. in the case of Inaba 35, of 50 mgm./kgm. in the case of the other two Inaba strains, and of 100 mgm./kgm. in Ogawa 41. These results are in agreement with those reported by Gauld *et al.*[2].

Five hours after the infection, 250 mgm./kgm. of chloromycetin was still effective in the case of the Inaba 29 and the Mediterranean Inaba strains (20 mice; 100 per cent survival), but was only partially effective against the Ogawa 41 and Inaba 35 strains (10 mice; 30 per cent survival).

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Caulobacter versus *Bacillus* spec. div.

The study of sessile freshwater bacteria induced Henrici and Johnson[1] to draw up a preliminary taxonomic system comprising many sessile species. They brought all these together in one order, Caulobacteriales; *Caulobacter* being the name given to one of the genera. This genus is characterized by the presence of a stalk connecting one end of the elongated bacterial body to a solid substrate and by multiplication by transverse binary fission. The first to describe such a bacterium, without giving it a name, was Jones[2].

Some strains, probably all to be classified as one species of this genus, have been isolated at Delft from tap water. The development of *Caulobacter* can be promoted by adding peptone water to the tap water so as to make a 0·01 per cent solution. The bacterium will then be found at the lower end of its stalk hanging down from the surface of the water. If a thin collodion film, such as is used as a specimen carrier in the electron microscope, is made to float on the surface, the bacterium attaches itself to the film. To obtain a preparation suitable for the electron microscope, one merely has to lift the film with a specimen carrier and to rinse it with distilled water[3].

Isolated in pure culture, *Caulobacter* may be cultivated on 1 per cent peptone agar; but single colonies always remain very small. In 1 per cent peptone water the bacteria produce irregular long forms; in 0·1 per cent peptone water more-normal single

individuals are obtained. Stalks, however, are very short in a pure culture, and growth is less abundant than might be expected.

Much better growth is obtained if the medium is inoculated simultaneously with some *Bacillus* sp. (for example, *Bac. subtilis* or *Bac. megaterium*). In a combined culture of *Caulobacter* and *Bac. subtilis* strain 'Marburg' in 0·1 per cent peptone water, the latter species is abundant after one day. Gradually, however, *Caulobacter* gains a dominating position. The bacillus serves as a support for *Caulobacter*: one, two, or as many as ten or more may settle on one cell of the bacillus. It is noteworthy that the spores of the bacillus always remain free from *Caulobacter*.

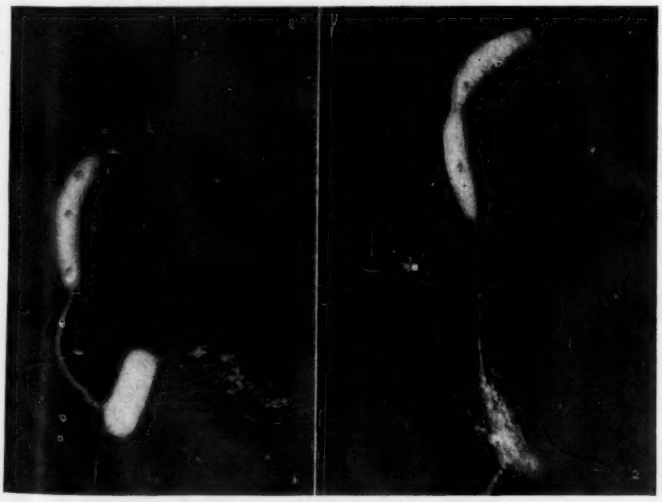
Electron micrographs give a better insight into the relationship of the two species. Occasionally a seemingly healthy bacillus with one *Caulobacter* attached to it is encountered (Fig. 1); most of the bacilli, however, look unhealthy. Finally, little but the cell membrane and the flagella—and in some instances only the flagella—is left (Fig. 2).

The fact that the micrographs give evidence that the stalk is tube-shaped, and apparently contains protoplasm, suggests that it acts as a sucking proboscis, which would imply that *Caulobacter* acts as a parasite to its host bacterium.

Caulobacter, when about to divide, grows a flagellum at its free end. The daughter cell thus is motile and swims rapidly before settling down. After this, a new stalk is formed. Since the flagellum of a sessile specimen, if present at all, is found to be inserted at the terminal end of the stalk, one may assume that a young *Caulobacter* attaches itself with the flagellated pole, and then grows away from the substrate it settled on by narrowing its basal end so as to form a stalk.

The *Caulobacter–Bacillus* relationship is certainly more complicated than can be described in this brief communication.

In more concentrated media, *Bacillus* species can adversely affect the growth of *Caulobacter* up to the point of suppressing it altogether; this is apparently due to chemical alteration of the medium by the metabolism of the bacillus, possibly including liberation of antibiotics.



Caulobacter parasitizing *Bacillus subtilis*. × 7,500

It is noteworthy that all evidence is in favour of the view that *Caulobacter* is not a cannibalistic bacterium.

A further account of these observations will be published elsewhere. This work is being supported by a grant from the Netherlands Organization for Pure Research (Z.W.O.) to the Technical Physics Department T.N.O. and T.H., Delft.

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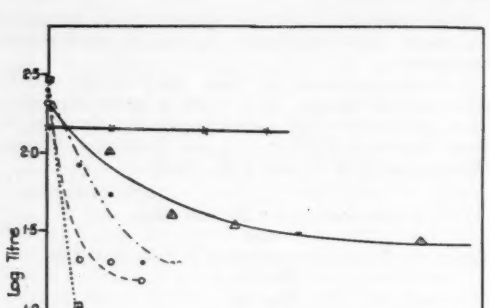
An Improved Diluent for the Production of African Horse-sickness Virus Vaccine

It is the general practice among virologists to use 10 per cent normal serum saline as the diluent of choice for the preparation of virus emulsions. In keeping with this practice, horse-sickness vaccine consists essentially of suitably prepared emulsions of selected virus strains in 10 per cent normal horse serum diluted in the same fluid to contain per dose an adequate number of infecting doses of these strains. Since the efficacy of the vaccine is dependent entirely upon the injection of living virus, it is essential that the diluting fluid should be one which favours longevity of the virus. Of a number of media tried, the 10 per cent horse serum saline was found to be the most desirable (Alexander, personal communication).

For various reasons, which will be discussed in a subsequent publication, it was decided to reinvestigate the problem. Tests were carried out using different concentrations of salts, salt mixtures, sugars, gelatine, amino-acids and egg-white. Viability of the virus on storage at 35° C. for periods of seven to twelve days as determined by *in vivo* virus titrations was used as the criterion of suitability of the various diluents.

It was found that solutions of inorganic salts and sugars have a pronounced lethal effect upon the virus. Gelatine and a number of amino-acids in a concentration of 0·1 per cent did not inactivate the virus; but none favoured viability to the same extent as 10 per cent serum saline. Ultimately, it was found that 10 per cent egg-white in *M*/50 phosphate buffer *p*H 7·5 was the diluent of choice.

In the accompanying graph are given the viability curves in different media of one strain of horse-sickness virus (strain 1,180), one of the least stable of the strains investigated. In the curves, the logarithm of the virus titre is plotted against the time in days of storage at 35° C. It will be seen that over a period of seven days the virus titre in 10 per cent egg-white phosphate buffer remained practically constant, whereas a variable reduction in titre down



to complete inactivation occurred with the other media tested. These results have been confirmed in other experiments.

This preliminary report is submitted in the hope that use of this medium may be of value to other workers in the field of viruses, particularly those handling the more fragile viruses such as influenza and rinderpest, or those engaged upon the preservation of virus emulsions by freeze-drying or other means.

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Erythrocyte Antibodies as a Cause of Abortion in Cattle

It is well established that bacterial antibody does not cross the bovine placenta, and that although bacterial antigens can leave the non-pregnant uterus and enter the blood stream[1], it is unlikely that they would normally leave the pregnant uterus. The barrier to antibody and antigen transfer is generally ascribed to the effect of the multi-layered epitheliochorial placenta of the bovine and the large size of the molecules concerned. Because of this it has been generally thought that hæmolytic disease of the fœtus was unlikely to occur in cattle; but hæmolytic disease of the new-born might follow sensitization of red blood corpuscles by colostral antibody as it does in the mare[2,3]. Although in cattle hæmolytic disease has not been recognized, there are a number of conditions of the fœtus such as some forms of early abortion, macerated fœtus, hydrops fœtalis, and premature still-birth, which have not been explained on other pathological grounds and which show certain features not altogether dissimilar to human hæmolytic disease. Normal agglutinogens comparable to the *ABO* groups of humans are believed to occur only rarely in cattle[4]; but a very large number of erythrocyte antigens have been defined by the production of immune sera in cattle and rabbits[5].

In view of these latter facts, and because it seems that another entrance of antibody to the fœtus than that via the vessels of the fœtal placenta may not be impossible[6], an investigation of the possible occurrence of hæmolytic disease in cattle was commenced. Sera from cows in several herds were tested by means of the ordinary *Rh* saline agglutination technique[7] against the red blood corpuscles of bulls to which they were or had been in calf. In the first herd (*A*), where a large number of abortions had occurred which had not been explained after thorough investigation, six positive sera out of forty-four were found. Four positive sera were found in a second herd (*B*) of ten animals where there had been long irregular œstrous cycles but no recognized abortions. A third group (*C*) of nine cows which were infertile, but the œstrous cycles of which had been regular, showed only one positive serum, and ten cows (*D*) which had all become pregnant at the first insemination from the same bulls as were used in herds *B* and *C* showed only one positive serum.

An immune serum was then made to the red blood corpuscles of a bull (*R*) which was of low fertility but which was producing apparently normal semen, and in which no infectious disease could be detected. This serum was produced in a heifer following successive intravenous injections of whole citrated blood. The maximum titre obtained was 1/160 in saline, and 1/640 in albumen[7]. The antibody-level fell rapidly, but could be raised by further injections, although not to the original high level. Tests with this serum indicated that the red blood corpuscles of the bulls *P* and *G* of herd *A* were probably antigenically similar to those of bull *R*. Two females, the red blood corpuscles of which were not agglutinated by the artificial immune serum, were mated to bulls *P* and *G* and became pregnant. Two others whose red blood corpuscles reacted similarly were mated and became pregnant to bull *R*. Sera from these animals were tested weekly as before against the erythrocytes of bull *R*. Three of them agglutinated the red blood corpuscles of bull *R* at some tests; but the reaction took place only in undiluted serum, except on one occasion in one animal when the titre rose to 1/2, and on another in another animal when it rose to 1/4. The remaining animal which was pregnant to bull *P* was transfused on the 100th and 115th days of pregnancy with 500 c.c. and 250 c.c. of citrated whole blood from bull *R* with no effect except a rise in the serum agglutinin titre to 1/16 after two weeks. The titre fell to 1/4 after a further week. This animal, and that pregnant to bull *G*, were each transfused on the 185th and 191st days of pregnancy respectively with 250 c.c. of citrated whole blood from bull *P*. Signs of commencing milk secretion were observed six days later in the former animal. The transfusions were repeated nine days later using 25 c.c. of blood for the animal pregnant to bull *P* and 225 c.c. for that pregnant to bull *G*. Two days later both animals aborted. No unlysed red blood corpuscles could be found in one fœtus. In the heart blood of the other there was an abnormally large number of erythroblasts. The serum agglutinin titre in both cases was 1/4 at abortion and rose after two weeks to higher than 1/32 (beyond which it was not titrated). The two animals which were pregnant to bull *R* were each transfused with 250 c.c. of his citrated whole blood on the 125th, 131st, 145th and the 103rd, 109th and 123rd days of pregnancy respectively. The serum titre of the former rose to 1/8 and she aborted eight days after the third transfusion. That

of the latter rose to 1/128 after the third transfusion and she aborted three days later. In each case the heart blood of the fœtus contained large numbers of erythroblasts.

Examination of the colostrum from these animals showed that the antibodies were concentrated by the udder. Typical examples are those where the serum titre was 1/4 and the colostral titre 1/20, and where the serum titre was 1/128 and the colostral 1/640. The colostral antibody was transferred to a calf's blood by feeding colostrum during the first two days of life.

Titration of the sera in saline against bull *R*'s erythrocytes with rabbit complement added gave hæmolysin titres similar to those of the agglutinins.

Throughout these experiments steps were taken to exclude infection and other factors as causes of abortion. The physical reaction to transfusion was in no case very great, being limited to a slight and transient dyspnœa and pyrexia, and it does not seem likely that abortion was due to transfusion *per se*.

From the evidence obtained it would appear that maternal circulating erythrocyte antibodies can cause abortion. The mechanism by which this is brought about may either be a reaction at the cotyledons or the result of an actual transference of antibody to the fœtal circulation. The antibody evidently becomes concentrated in the colostrum and is capable of being absorbed by the new-born calf. The interpretation of the full significance of these observations in relation to disease of the fœtus and young calf must depend on further investigation.

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Transfer of Bacterial Antibodies from the Hen to the Chick

During an investigation on the transfer of antibodies against *Salmonella pullorum* from the hen to egg yolk, non-agglutinating antibodies as well as agglutinins were found in the hen's serum and in the yolk. The non-agglutinating antibodies were detected in serum by the method of Morgan and Schutze[1]. The serum was diluted serially, and an equal volume of antigen added to each dilution; after 18 hr. at 53° C. any agglutination was noted, and the antigen in those tubes in which there was no agglutination was washed; to each of those tubes was added anti-avian-globulin serum. If the antigen had been sensitized by the unknown serum, it was agglutinated in the presence of the anti-avian-globulin serum. Antibodies of this type are referred to as non-agglutinating antibodies.

Egg yolk was tested for the presence of this type of antibody by first preparing an extract of the yolk. After exposing the antigen to serial dilutions of this extract, it was mixed with the serum of a rabbit which had been inoculated with yolk livetin. Samples of both serum and egg yolk from a small flock of hens that had been inoculated intravenously with killed *Salm. pullorum* were tested for antibodies. The titres of non-agglutinating antibodies in both sera and yolks were 7 to 34 times greater than the corresponding agglutinating titres.

A similar experiment has been carried out in which serum and yolk samples from three groups, each of four hens, inoculated either intravenously, subcutaneously or intramuscularly, were examined for agglutinating and non-agglutinating antibodies. In hens inoculated intravenously, the titres of non-agglutinating antibodies in serum and in yolk were approximately equal, but the agglutinin titre of yolk was much lower than that of serum. The sera of the hens inoculated by the subcutaneous and by the intramuscular route contained agglutinating and non-agglutinating antibodies to about the same titre as in those inoculated intravenously; but there was practically no antibody of either kind demonstrable in yolk. The difference in the titres of the yolks of eggs from the hens inoculated by the subcutaneous or intramuscular route as compared with the intravenous route was consistent and very marked. During an experimental period of sixty days the yolks of seventy-six eggs from intravenously inoculated hens all had non-agglutinating titres varying from 80 to 2,560. During the same period more than ninety eggs from hens inoculated intramuscularly and subcutaneously were similarly examined; the yolks of the majority of these eggs had titres of less than 10 and the remainder had titres of only 10 or 20.

Preliminary observations on a limited number of embryos and newly hatched chicks have indicated that antibody passed from the egg to the chick is largely of the non-agglutinating type.

The method by which protein accumulates in the rapidly developing hen's ovule is not clearly understood[2]; but there is evidence to suggest that the protein molecule is hydrolysed by proteolytic enzymes as it passes through the follicular epithelium into the ovule[3]. Similarly, the transfer of protein from the yolk to the developing embryo is associated with proteolytic enzymes in the yolk, particularly cathepsin, which reaches maximum activity at about the fifteenth day of incubation. Rosenheim[4] has shown that under suitable conditions trypsin, pepsin and papain destroy *Salm. typhosum* 'O' agglutinins in horse serum.

With these considerations in mind, experiments have been carried out on the action of trypsin on both

Effect of Trypsin on Agglutinating and Non-agglutinating Antibodies in Hen Sera

Serum No.	Test	Control	Concentration of trypsin: 0·005 per cent	0·05 per cent	1·0 per cent	2·0 per cent	4·0 per cent
1007	*A*	40	10	10	0		
	NA	80	80	20	0		
1004	*A*	80	0	0	0		
	NA	>160	80	80	0		
1285	*A*	640	640	640	0	0	0
	NA	20,480	20,480	20,480	20,480	20,480	10,240
1280	*A*	160	160	160	0	0	0
	NA	5120	5120	5120	5120	2560	5120

A = titre of agglutinating antibody; *NA* = titre of non-agglutinating antibody.

agglutinating and non-agglutinating antibodies to *Salm. pullorum* in hen's serum. The results of some of these experiments are shown in the accompanying table. By mixing a suitable concentration of trypsin with serum and incubating at 37° C. for one hour at a *p*H of 8·5, it was possible to reduce and even destroy all demonstrable agglutinins without affecting the titres of non-agglutinating antibodies.

Details of experiments on the transfer of antibodies from the hen to the chick via the egg and the relationship between agglutinating and non-agglutinating antibodies will be published elsewhere.

I am much indebted to the Scientific Director of the Animal Health Trust and to the Director of the Trust's Poultry Research Station for their kindness in releasing me from my routine duties to undertake this work in Cambridge.

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Glucose and Lysogenesis in *Bacillus megatherium*

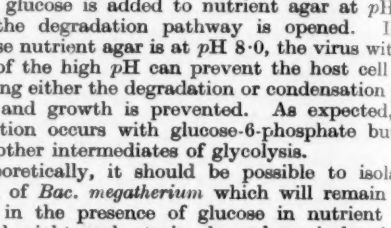
Cells of *Bacillus megatherium* strain 1·0 can carry on metabolic activity, including multiplication, simultaneously with virus formation, for an infinite time; this balance between the cells and their virus is called lysogenesis. Evidence that lysogenic *Bac. megatherium* strain 1·0 can return to the normal uninfected state on a glucose nutrient medium was presented by Ehrlich and Watson[1]. Further study of this phenomenon showed that other sources of carbon, such as fructose, maltose, sucrose, glycerol, pyruvate and succinate in the basal medium all cause recovery from the lysogenic state. In postulating an explanation for this behaviour, it was thought that the virus and the source of carbon might compete for organic phosphate and that the virus was the weaker competitor. The work of Kozloff and Putnam suggested such a hypothesis[2]. However, initial studies showed that this hypothesis could not explain the effect of glucose on lysogenic *Bac. megatherium* strain 1·0.

Glucose in alkaline nutrient agar at *p*H 8·0 was found to inhibit the growth of the lysogenic strain, whereas the uninfected parent strain was not inhibited. Of other sources of carbon that were added to the alkaline medium, only glucose-6-phosphate had an inhibitory effect on growth of the lysogenic strain. Fructose, fructose-6-phosphate, hexose diphosphate, α-glycerophosphate and glycerol did not inhibit growth. Evidence that at least some of these intermediates of glycolysis can be utilized rests in part on the fact that resting-cell suspensions oxidized hexose diphosphate, glucose-6-phosphate, fructose-6-phosphate and phosphoglycerate. Also, 0·01 *M* sodium fluoride prevented the formation of lactic acid from glucose by resting-cell suspensions of uninfected *Bac. megatherium*.

These results suggest the following explanation for the effect of glucose. When glucose is present in a medium that contains a full complement of organic nitrogen compounds, as in nutrient agar, deoxyribonucleic acid might be synthesized via a degradation of glucose-6-phosphate to gluconate-6-phosphate, arabinose-5-phosphate, deoxyribose-5-phosphate, and deoxyribose-1,5-diphosphate, thence to nucleotides and nucleic acid. Such a degradative pathway has been suggested, at least in part, by Dickens[3]. If glucose is present in a synthetic medium, or if the cells are grown on nutrient agar without glucose, the deoxyribose in the deoxyribonucleic acid is probably synthesized by a condensation pathway involving two and three carbon compounds. In fact, Racker[4], and Marmur and Schlenk[5] have shown that certain bacteria can form pentose by a condensation of two and three carbon compounds. Thus, if the virus in lysogenic strain 1·0 of *Bac. megatherium* parasitizes the condensation pathway to deoxyribose and not the degradation pathway, lysogenicity will be lost when glucose is added to nutrient agar at *p*H 7·0, and the degradation pathway is opened. If the glucose nutrient agar is at *p*H 8·0, the virus with the help of the high *p*H can prevent the host cell from utilizing either the degradation or condensation pathway, and growth is prevented. As expected, this inhibition occurs with glucose-6-phosphate but not with other intermediates of glycolysis.

Theoretically, it should be possible to isolate a strain of *Bac. megatherium* which will remain lysogenic in the presence of glucose in nutrient agar. Indeed, eight such strains have been isolated from nutrient agar containing 0·01 *M* glucose. These strains yield virus after repeated transfers on glucose nutrient agar and are resistant to infection with stock virus. However, these strains remain lysogenic on nutrient agar without glucose.

Proposed Pathways for the Synthesis of Deoxyribonucleic Acid by the Host Cells of *Bacillus megatherium*



* Parasitized by virus of strains that remain lysogenic on glucose agar but not of strain 1·0.
† Parasitized by virus of lysogenic strain 1·0.

Thus it is suggested that different mechanisms may exist in the host cell of *Bac. megatherium* for the synthesis of deoxyribonucleic acid and that virus can parasitize one or more of these mechanisms (see chart). Thus, if the mechanism of synthesis of deoxyribonucleic acid that is utilized by the host can be parasitized by the virus, the virus is synthesized and the cells are lysogenic; but if another pathway is made available, for example, when glucose is added to nutrient agar, the alternative pathway will be used and lysogenesis will be lost.

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Occurrence of Antifungal Substances in *Brassica rapa*, *Brassica oleracea* and *Beta vulgaris*

SINCE roots and tubers contain different substances which are easily assimilable by many micro-organisms, the question arises whether the mechanical protection by the few outer cell layers of the cortex is sufficient to prevent infections from the surrounding soil, or whether the plant produces antibacterial and antifungal substances to increase its resistance against microbial invasion.

Antifungal substances have been isolated from the tubers of *Brassica rapa rapifera*, *Brassica oleracea gongylodes* and the fleshy roots of *Beta vulgaris*. In the first two cases, the presence of the active substances was strictly limited to the superficial cell-layers, whereas the interior parts were practically free from it; this seems to point to the hypothesis of antimicrobial protection referred to earlier.

In the case of *Brassica rapa* the substance, which we have called 'rapine', could be isolated by disintegrating the peelings in a blendor and subjecting the mass to steam distillation. From the distillate a yellow oily substance could be extracted with ether; the yield amounted to 4 mgm. per 100 gm. of fresh peelings. The substance had the characteristic penetrating smell of turnips. The solubility in water was low and, mixed with malt agar, a concentration of $1:10^5$ was sufficient to prevent the growth of different fungi and yeasts. At this concentration, rapine did not possess any antibacterial activity against *Escherichia coli*, *Micrococcus aureus* and *Bacillus subtilis*. This specific antifungal activity of rapine was used to check so-called pure cultures of phytopathological fungi for the presence of bacteria, which otherwise is sometimes rather difficult.

As *Trichophyton rosaceum*, a typical parasite of man and various animals, is readily inhibited by rapine, the pharmacological activity of this substance will be a matter of further examination. The following fungi were tested: *Aspergillus niger*, *Aspergillus oryzea*, *Botrytis* sp., *Ceratostomella pififera*, *Fusarium oxysporum*, *Fusarium solani*, *Penicillium notatum*, *Rhizopus nigricans*, *Trichophyton rosaceum* (growing on Sabouraud agar), *Verticillium cinnabarinum*. Of these, *Fusarium oxysporum* proved to be the most susceptible. The yeasts tested were: *Saccharomyces cerevisiæ*, *Saccharomyces lactis* Dombrowski, *Schizosaccharomyces* Pombe, *Cryptococcus dattila* and *Cryptococcus utilis*, all of which were readily inhibited by a concentration of $1:10^5$ of rapine.

The active principle from *Brassica oleracea gongylodes* was prepared in the same way as rapine. It proved to be a yellowish, solid substance, also slightly soluble in water and with an intense smell of Swedish turnips. It inhibited the growth of the same micro-organisms as did rapine; its activity, however, seemed to be somewhat less pronounced, as in the cases of *Penicillium notatum* and of *Fusarium oxysporum* the critical concentration was $1:10^4$.

From *Beta vulgaris* a crude preparation was made which was especially inhibitive against *Fusarium* sp. This substance was not volatile with steam, was not deteriorated by boiling, was soluble in water and insoluble in ether. It could be extracted from the cortex as well as from the inner part of the root.

Further studies in this field are in progress.

Thanks are due to the Central Bureau voor Schimmelcultures, Baarn, for supplying us with pure cultures of a number of the above-mentioned fungi.

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'Lactobacillin', an Antibiotic from Lactobacilli

A SURVEY of streptococci has already shown that the lactic group are the most powerful antibiotic producers[1]. It was of interest to see whether antibiotics are also produced by lactobacilli, especially since Grossowics, Kaplan and Schneerson[2] have already reported the existence of such substances. Young Gruyère cheese samples from different areas of France were used as one of the sources of these lactobacilli. 40 antibiotic-producing strains, about 3 per cent of the total number of strains tested, have been isolated in pure culture. These gave zones of inhibition 20–30 mm. in diameter when assayed by the cylinder plate method with *Staph. aureus* as the test organism.

One of these organisms, which is homofermentative and resembles *L. helveticus*, is being investigated in further detail. In peptone–glucose broth, containing 0·01 per cent Tween 80, which appears to be essential, supplemented with tomato juice, whey or malt extract, it grows well at 30° C., 37° C. and 43° C.; the lower temperatures are, however, more conducive to good yields of antibiotic. Yeast–glucose broth supports good growth at 43° C., but the yeast extract is inhibitory to growth at lower temperatures, and inhibits production of antibiotic at all temperatures of incubation.

With dilution assays in plain nutrient broth, in which the test organism grows well but the lactobacillus does not grow, titres of 1/1,280 were frequently obtained. Against *Staph. aureus* this culture-fluid appears to be about twenty times more powerful than nisin[3]; it is, however, very heat-labile.

We are greatly indebted to G. Mocquot, of the Station Centrale de Technologie Agricole, Paris, for supplying the samples of Gruyère cheese.

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Urease Metabolism in *Citrullus*

SINCE the speculations by one of us[1] as to the function of urease in *Citrullus* seeds appear to have aroused interest, we feel we ought to report that further work has shown the original hypothesis (that urease is a reserve protein) to be untenable. We remain unconvinced that the metabolism of urease in *Citrullus* is connected with the metabolism of urea or its derivatives; but it does not appear to be a

normal seed-reserve. First, we find that the protein content of the cotyledons of seedlings grown in a nitrogen-free culture medium may fall in eight days to about half its initial value; but the urease content may remain unchanged. Secondly, if seedlings are grown under optimal conditions in soil, we find in the cotyledons no appreciable change in major nitrogen fractions over ten days; but a reduction in urease begins to be apparent at about the fourth–sixth day, followed by a rapid fall almost to zero at about the eighth day. The rapid rise in water-content from about the fourth day shows that this is the time when cell-extension and vacuolation begin; and since Brown and Broadbent[2] have shown that this is a time of considerable metabolic activity, it seems likely that disappearance of urease is connected with its onset.

Moreover, examination by Granick's[3] histochemical method shows that the urease is retained in the bundle-sheaths (the cells of which enlarge relatively little) long after it has disappeared from the mesophyll; detached cotyledons, which expand more slowly than do cotyledons attached to the seedling, also lose their urease more slowly; and etiolated seedlings, the cotyledons of which do not expand, retain their urease for at least thirteen days. In general, the urease metabolism appears to be bound up with the development of the cotyledons rather than with that of the seedling as a whole; and we are attempting to identify more closely the particular process with which it is associated.

We should like to express our gratitude to the Ferry-Morse Seed Co. of California for their generous gift of the seeds used in this investigation.

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Extraction of Chloride from Mammalian Skin

In the course of an investigation into the distribution of chloride between blood plasma and skin in the cat, difficulty was encountered in determining the chloride content of the skin. Digestion of the tissue in concentrated nitric acid is impracticable owing to loss of chloride; the tissue proved to be too tough to be ground in trichloracetic acid, which is the standard procedure with other tissues. Freezing before grinding had no helpful effect and cutting the tissue into very small fragments before grinding was, though successful, both laborious and time-consuming; mincing was impracticable owing to the relatively small samples which could be used.

By chance, it was found that a sample of skin which had been dried for 18–24 hr. at 105–110° C. for determination of its water content was quite readily macerated if left in 10 per cent trichloracetic acid a few minutes before being ground. The same effect was produced by drying cat's skeletal muscle, which in the fresh state is much more resistant to maceration than is frog's muscle. On the other hand, liver and kidney, which can be readily macerated in the fresh state, were considerably toughened by the drying process. Thiocyanate was also stable in skin during the drying process and could be determined in the trichloracetic acid filtrate. Since the ease with which a tissue can be handled undoubtedly affects to some extent the amount of attention which is bestowed upon it, the observation now made may be of use to others.

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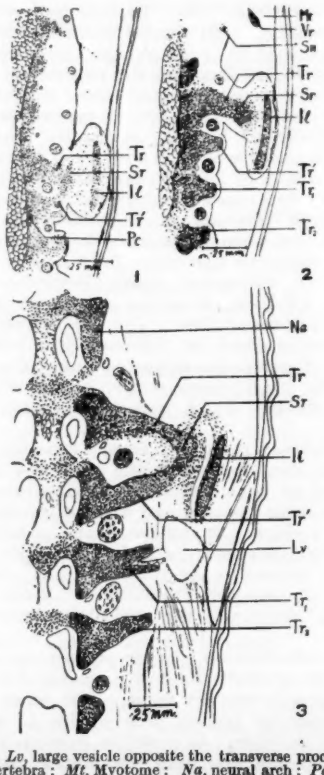
Sacral Ribs of Lacertilia

The sacral ribs of Lacertilia have been for many years the subject of discussion by zoologists. Some consider that the transverse processes of the sacrum are homologous with the ribs of the presacral region[1,2]. Siebenrock[3] made a thorough study of this subject and came to the conclusion that the first sacral vertebra of Lacertilia possesses ribs instead of transverse processes. Moodie[4], however, states quite clearly that there are no sacral ribs in modern lizards. According to him lizards "occupy an isolated place among all other known reptiles in not having any sacral ribs whatever". He considered the processes carried by the sacral vertebræ as transverse processes and not ribs. A similar conclusion was arrived at by Huene[5]. Later, Moodie[6] referred to this view again,



Il, Ilium; *Lv*, large vesicle opposite the transverse process of the first tail vertebra; *Mt*, Myotome; *Na*, neural arch; *Pc*, primary centrum; *Sn*, spinal nerve; *Sr*, sacral rib; *Tr*, *Tr'*, *Tr*$_1$, *Tr*$_2$, transverse process of the sacral, secondary sacral, first caudal and second caudal; *Vr*, ventral rib

and suggested that a study of the stages of chondrification might show evidence of the presence of sacral ribs in lizards. In spite of this suggestion by Moodie, most workers in this field have accepted Moodie's earlier conclusion as definite and maintained that there are no sacral ribs in Lacertilia. El-Toubi[7], however, has reported evidence of sacral ribs.

In view of this controversy, the study of the ribs of the sacral region of *Chalcides ocellatus* was suggested by the late Prof. Naef. Different embryos of *C. ocellatus* varying in length from 14 to 45 mm. have been studied in transverse as well as in horizontal sections. I find that the sacral ribs are quite distinct and separate from the transverse processes (Fig. 1). Each rib is a compound one formed from a dorsal rib rudiment and a ventral rib rudiment which fuse early in the connective tissue stage, forming the sacral rib, which extends downwards and backwards. Thus, the sacral rib chondrifies and becomes united with the transverse process of the sacral vertebra (Fig. 2).

At the same time the transverse process of the secondary sacral vertebra extends anteriorly and downwards and thus becomes connected with the sacral rib. Thus the two transverse processes of the sacral and the secondary sacral become united by the sacral rib, which articulates with the ilium (Fig. 3). A detailed account of this work will be published elsewhere.

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Structure of $Cu_{32}Al_{19}$

POWDER photographs show that the deformed gamma structure $Cu_{32}Al_{19}$ has 51 atoms in each of the pseudo-cubic unit cells; it is closely related to the cubic structure of Cu_9Al_4 with 52 atoms per unit cell[1]. Whereas the latter is found with 21/13 or 22/13 electrons per atom, the deformed structure takes up to 89 electrons to 51 atoms. The complex quantities may be approximately simplified to the ratios 5/3 and 7/4 respectively; these improper fractions correspond to the monotonic sequence[2] $(2n-1)/n$. (Cu is monovalent, Al trivalent.)

The loss of atoms follows the simple rule that each additional electron per unit cell replaces one atom[4]. It is achieved by the substitution of one atom of aluminium for two atoms of copper. Thus we have the following sequence:

Formula	Symmetry	Atoms	Electrons
$Cu_{34}Al_{18}$	cubic	52	88
$Cu_{33}Al_{18\frac{1}{2}}$	monoclinic	$51\frac{1}{2}$	$88\frac{1}{2}$
$Cu_{32}Al_{19}$	monoclinic	51	89
$Cu_{31}Al_{19\frac{1}{2}}$	rhombohedral (?)	$50\frac{1}{2}$	$89\frac{1}{2}$
$Cu_{30}Al_{20}$	rhombohedral (?)	50	90

The total number of atoms and electrons remains constant at exactly 140. The numbers of atoms in the monoclinic phase falls from $51\frac{1}{2}$ to 51, while the rhombohedral phase contains $50\frac{1}{2}$ or 50 atoms per unit cell. The Brillouin zone is the same as for Cu_9Al_4, the number of electrons per unit cell rising to a maximum of 90, which agrees exactly with the number of states required to fill the first zone.

All the copper and aluminium atoms in $Cu_{32}Al_{19}$ have definite locations, and it is this fact which leads to the abnormally low symmetry. The extremely peculiar mode of atomic distribution cannot possibly be explained by the size factor, as copper and aluminium atoms are too nearly equal for it to have any effect. The real explanation is to be sought in the electronic factor, each individual free valence electron occupying a definite lattice site[3]. Thus for a complete elucidation of the structure, it is necessary to locate 32 copper atoms, 19 aluminium atoms and 89 electrons.

So far as can be ascertained at present, it appears that the free valence electrons are grouped around the aluminium rather than the copper ions, so that on the whole each aluminium ion retains three negative charges while each copper ion retains one negative charge, but they are usually shared by neighbouring atoms. Thus the structure remains electrically neutral at every point,

The space-group is in all probability Cm, and this means that the crystal unit must be doubled, a second set of 51 atoms being required in order to complete the full symmetry with a translation (000, $\frac{1}{2}\frac{1}{2}$0). The true monoclinic cell of $Cu_{32}Al_{19}$ therefore contains 102 atoms, of which eleven groups of four atoms and ten groups of two atoms are copper and seven groups of four atoms and five groups of two atoms are aluminium. (These groupings are subject to confirmation by a complete structure analysis.)

The symmetry degradation is most easily considered as occurring in stages, thus:

$$P\,\bar{4}3\,m \rightarrow P\,\bar{4}2\,m \rightarrow C\,\bar{4}\,m\,2 \rightarrow C\,mm \rightarrow C\,m.$$

The pseudo-tetragonal indices of the cubic line 72 (660, 822) on the cobalt powder photograph are readily ascertained. There are three successive reflexions comprising $660\,\alpha_1$, $(660\,\alpha_2 + 606\,\alpha_1)$ and $606\,\alpha_2$. 822 is too weak to be observed. For tetragonal symmetry ($P\,\bar{4}2\,m$) line 48 (444) would not be resolved, but in the orthorhombic unit cell ($C\,mm$) this becomes 804 and 084 and the reflexions are blurred. A detailed examination is required using vanadium radiation, in order to obtain the exact dimensions of the unit cell, and the monoclinic angle (β).

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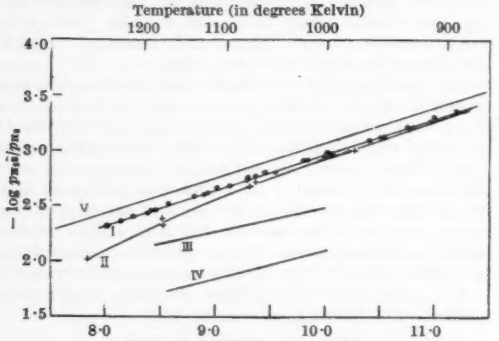
Thermodynamics of Ferrous Sulphide

THE free energy of formation of β-ferrous sulphide has recently been investigated by us as part of a wider study of the thermodynamics of metal sulphides and sulphide mixtures. In view of the metallurgical importance of ferrous sulphide, the results so far obtained are reported here, although it is intended to publish the results of the whole investigation when completed.

The procedure adopted was to measure the composition of gas mixtures of hydrogen and hydrogen sulphide in equilibrium with powdered iron and ferrous sulphide heated to temperatures ranging from 720° to 1,258° K. The gases were circulated over the solids in a closed system, at such a rate that the steady concentration of hydrogen sulphide produced was unaffected by a twofold change in the flow rate, in either direction. Errors due to thermal segregation were therefore avoided.

Radiosulphur was used so that the gases could be analysed continuously by gas counting. It was thus easy to follow the attainment of equilibrium, and the effect of rate of flow on the segregation of the gases in traversing the thermal gradient from the even zone of the furnace to the counter, which was at room temperature. It was found that, even at the lowest temperature used, equilibrium between the gas and solid phases was rapidly attained.



I, Present work; II, Cox, Bachelder, Nachtrieb and Skapski; III, Jellinek and Zakowski; IV, Britzke and Kapustinski; V, Kelley

The results above 890° K. are shown in the accompanying graph, together with those of other workers who have used this equilibrium[1-3]. The values of p_{H_2S}/p_{H_2} are considered accurate to ± 3 per cent, the errors in the counting, analysis and temperature measurement all being included. When combined with data for the free energy of formation of hydrogen sulphide[4], our results yield the following equation for the free energy of formation of two moles of β-ferrous sulphide from iron and diatomic sulphur gas,

$$\Delta G^0_{720-1,261^\circ K.} = -71,820 + 25{\cdot}12\,T \text{ cal.}$$

It will be seen that the results of Cox, Bachelder, Nachtrieb and Skapski[1] agree closely with ours, except at the higher temperatures. We consider that this difference is mainly due to thermal segregation at their rate of flow[5], the error attributable to this being 25 per cent in the H_2S/H_2 ratio at 1,200° K. Under static conditions, the error due to segregation would be about 100 per cent at 1,200° K., and this would be sufficient to account for the results of Jellinek and Zakowski[2] at 1,183° K. We cannot account for their results at 1,003° K., or the results of Britzke and Kapustinski[3], on the basis of thermal segregation errors alone.

Kelley[4] has developed an equation for the free energy of formation of ferrous sulphide on the basis of the heat of formation and heat capacity data available in 1936. Curve V in the figure shows that the results derived from this are in fair agreement with our own.

The work reported here was done when we were on the staff of the British Iron and Steel Research Association, and we are indebted to its Director for permission to publish these results.

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Rotatory Dispersion Measurements with a Unicam Spectrophotometer

Photo-electric polarimeters are not yet available commercially, although several types have been described in recent years[1]. Between about 4500 and 6500 A. there is no particular advantage in using such instruments, since in this region visual spectropolarimeters are very accurate and convenient; but for longer and shorter wave-lengths, photo-electric methods are less tedious than the older photographic ones. We are interested in the red and near infra-red for the extension of studies on the Cotton effect for blue nitroso compounds[2], and we set out to adapt a Unicam spectrophotometer for measuring rotatory dispersion by adding two Glazebrook prisms and a graduated circle. The arrangement has proved to be so satisfactory for our work that we feel justified in making this preliminary communication.

In the Unicam instrument there is a passage (between the monochromator and the cell compartment) into which the polarizing prism can be fitted, but the cell compartment is not long enough to house the other prism as well as the observation cell. Accommodation for the analyser is therefore provided in a separate unit which is constructed so as to make the apparatus completely light-tight. The prism is mounted in a rotatable tube between the central aperture of the graduated circle and a similar hole cut in a rectangular plate held to the circle by three pillars. Rotation of the prism is controlled by an arm which carries a vernier reading to 0·02°. When the analyser unit is inserted in the instrument, the rectangular plate forms a new back for the cell compartment, and the graduated circle is fixed in front of the photocell box.

The polarizer and analyser are adjusted visually so that when they are crossed the scale reads approximately 0°. The actual zero is found by the 'method of symmetrical angles'[3] in the following way: (1) place cell containing solvent in light beam; (2) turn transmission scale to 100 per cent; (3) set wave-length scale to desired value; (4) set analyser to a stop fixed at − 40° (the most convenient 'half-shadow angle'); (5) adjust slit-width to give balance; (6) rotate analyser clockwise through about 80° to a position θ_0 to restore balance, then the zero is $\frac{1}{2}(\theta_0 - 40)$.

Readings of rotation are made in a similar way: (1′) place cell with solution in light-beam; (2′) adjust transmission scale. If the solution is transparent, the scale is left at 100 per cent; but if absorption is present it may be necessary to select a smaller value to prevent the slit-width from becoming too large.

Operations (3′), (4′), (5′) and (6′) are then carried out as for (3), (4), (5) and (6). If in (6′) the analyser scale reads θ, the desired rotation α is given by

$$\alpha = \tfrac{1}{2}(\theta - 40) - \tfrac{1}{2}(\theta_0 - 40) = \tfrac{1}{2}(\theta - \theta_0).$$

The accompanying table contains values of rotation α and specific rotation $[\alpha]$ for various wave-lengths λ, obtained with a benzene solution of caryophyllene nitrosite (concentration 1·5280 gm. per 100 ml.) in a 1-cm. cell.

λ	α	$[\alpha]$	ϵ
5000	+ 1·47°*	+ 962°	0·30
5500	+ 1·93 *	+ 1293	1·38
5800	+ 2·40 *	+ 1571	3·53
6000	+ 2·67	+ 1748	6·17
6250	+ 2·74	+ 1794	10·50
6500	+ 2·42	+ 1584	15·40
6750	+ 0·94	+ 615	21·21
6800	+ 0·41	+ 268	21·99
6900	− 0·61	− 400	21·05
7000	− 1·96	− 1283	18·46
7250	− 3·05	− 1996	10·14
7350	− 3·07	− 2009	6·89
7500	− 2·82	− 1846	3·25
7750	− 1·84	− 1204	0·77
8000	− 1·30	− 851	—
8500	− 0·76	− 498	—
9000	− 0·49	− 303	—

* Visual readings.

It is seen that the specific rotation (column 3) passes through positive and negative maxima at about 6250 A. and 7350 A., respectively. Previous visual measurements had revealed only the positive maximum. Molecular extinction coefficients ϵ (also found with the Unicam spectrophotometer) are included in the table (column 4). Maximum absorption occurs near 6800 A. where $[\alpha] \simeq 0$.

The analyser unit described above was constructed in our workshop under the direction of Mr. Robert McCulloch. One of us (J. V.) is indebted to the Department of Scientific and Industrial Research for a maintenance allowance.

STOTHERD MITCHELL
JAMES VEITCH

Chemistry Department,
University of Glasgow.
June 1.

[1] See Weissberger, "Physical Methods of Organic Chemistry", 2, 967 (New York, 1946).
[2] Mitchell, *J. Chem. Soc.*, 3258 (1928) and subsequent papers.
[3] Kenyon and Dickes, *Nature*, 117, 304 (1926).

Modification of Crystal Habit of Inorganic Salts with Dyes

THE publication of H. E. Buckley's "Crystal Growth"[1], with its chapters on modified crystals, is likely to stimulate interest in this field of research; the time is therefore opportune for this indication to be given of a conception of modification of crystal habit with dyes which, so far as it is at present capable of being tested, appears to explain many of the contradictions and inconsistencies previously confronting workers in this field.

Recent evidence points to the growth at finite rates of crystal faces being due to the spreading of layers from continuously renewed nuclei on the faces[2]. It is now suggested, from study of some forty cases of strong crystal-habit modifications of anhydrous inorganic salts, including nitrates, sulphates and perchlorates in particular, that the incorporation, temporary or permanent, of dye molecules in one or more of the edge-faces of these layers is the usual means by which the growth of a crystal face is retarded by the presence of a dye in the solution[3,4]. Alternatively, the propagation of the nucleus could be affected in a similar manner[5].

The edge-planes of these spreading layers may conceivably be vicinal faces of high indices, or the ordinary low-order planes of high reticular density normally bounding the crystal, or something in between, according to their stage of growth. Consideration of many individual instances of very strong crystal-habit modifications suggests that in most cases the dye is incorporated into the edges of such spreading layers by virtue of there being suitable coincidence between the dye's polar groups and ionic sites in a particular edge-plane of the layer, so that the dye molecule can be incorporated in the edge-plane by virtue of its polar groups replacing the appropriate ions. The edge-plane involved will depend to some extent on the orientation of the polar substituent groups of the dye molecule; alternative edge-planes may thus be involved in bringing about crystal-habit modification on one particular face. The concept is virtually of a 'two-dimensional solid solution formation' at the growing edge of a layer. It is evident that a planar dye molecule will lie entirely in one crystal plane, and where the dye molecule preferentially fits or can only fit in one plane, then favourable conditions for the exhibition of pleochroism by the completed crystal exist, if all the dye molecules are not dislodged in the growth process.

In order that a dye may have strong crystal-habit modifying properties for a given salt, it should have the following properties. (1) Its solubility in the saturated saline solution should be high; this implies that an adequate number of solubilizing polar groups should be present. (2) The charge centres of the polar groups should coincide accurately with sites for similarly charged ions in a crystal plane; the greater the number of polar groups fitting into the lattice, the stronger the adhesion of the dye. (3) Any such polar group should not occasion disturbance of the lattice about the ionic site in question; any exclusion of adjacent ions will result in a diminution of the Coulomb forces responsible for the adhesion of the dye molecule. Hence arises the importance of a suitably close coincidence between the oxygen triangles of sulphonate groups and oxyacid ions.

Since presumably a certain amount of misfit may be tolerated in some cases at least, this conception of habit modification is quite consistent with the large number of dyes found by Buckley to have habit-modifying properties in various degrees. Further, it is evident that a certain selectivity will be exercised by the crystal structure, and not all dyes will fit; and that the effective dyes will depend on the crystal structure to be modified. Differing types of habit modification of one substance will depend on the different planes on which the dye molecules can fit.

Since commercial dyes are often complex, and erroneously described in the Colour Index, only strong habit modifications obtained with specially prepared pure dyes of known simple constitution have been considered. It has been possible to demonstrate by graphical methods that the requisite accurate coincidences between polar groups and ionic sites are, in fact, possible on planes indicated by the pleochroism of the dyed crystals as being the seat of the dye molecules, in such cases as the habit modification of ammonium nitrate IV with naphthol red *S*, acid magenta and 1 : 4 diamino anthraquinone 2-sodium sulphonate, for example. In certain cases, where the plane including the dye molecules is

multiplied in the crystal by symmetry operations, and as a result the dye molecules are not parallel (for example, in the trigonal and hexagonal systems when modification is on the basal plane), pleochroism of dye inclusions in the modified crystals cannot be expected. Where it has not been possible to obtain crystals with pleochroic adsorbed dye inclusions, the necessary coincidences between the polar groups of dyes and ionic sites of suitable planes to account for the observed habit modifications may be similarly demonstrated. It is perhaps noteworthy that a small minority only of the habit modifications examined appear to be capable of explanation by fitting the dye molecule directly into the habit-modified face (for example, potassium nitrate with acid magenta). In these cases the habit modification is apparently not so strong as that found when the same dye fits on to a plane perpendicular to a habit-modified face of another salt; close comparisons are not justifiable, however, when solubility differences exist.

Some attempts are being made to predict the structures of crystal-habit modifiers according to this theory. In a certain number of cases success has been achieved; but a difficulty has arisen, that it is impossible to predict the solubility of the dye, which depends just as markedly on the orientation of the polar substituent groups as does its crystal-habit modifying powers[6]. A full account of this work will be reported elsewhere.

J. Whetstone

Research Department,
Nobel Division,
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Stevenston, Ayrshire.
May 31.

[1] Buckley, H. E., "Crystal Growth" (John Wiley and Sons, 1951).
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Contamination of Evaporated Films by the Supporting Material of the Source

In studies of the properties of films produced by vacuum evaporation, it is important to know whether appreciable contamination by the material of the boat or filament occurs under the normal evaporation conditions. The mass of a film is usually very small, and some properties of films are very sensitive to minute amounts of contamination. A test of high sensitivity is therefore required. The radioactive tracer method possesses this necessary sensitivity when there is a suitable isotope of the material of the boat.

The mechanical properties of tantalum and its high melting point make it a suitable material for evaporation sources. It has an active isotope, tantalum-182, with a half-life of 117 days. Moreover, since the whole of natural tantalum consists of the 181 isotope, from which the active isotope is produced by neutron irradiation, a high specific activity is easily obtained. Experiments have been made with an active tantalum boat as evaporation source, the evaporated materials being tested for active tantalum in the usual way. Silver and germanium have been examined, the evaporation being carried out at several temperatures. One count per minute above background corresponds to $2{\cdot}3 \times 10^{-10}$ gm. tantalum in the deposit. The masses of silver and germanium deposited on the test target were of the order 10^{-4} gm., so that contamination to the extent of a few parts per million was detectable.

The results with germanium showed that the tantalum content of the film ranged from 11 p.p.m. for an evaporation temperature of 1,120° C. to 26 p.p.m. at 1,800° C. For silver, the figures recorded were from 20 p.p.m. at 1,100° C. to 113 p.p.m. at 1,670° C.

The mechanism by which the tantalum is transported is not clear, since the two obvious processes, namely, direct evaporation and solution by the molten metal, would be much more temperature-dependent. From the practical point of view, however, it is seen that provided the temperature of the source is kept below about 1,200° C., the contamination does not exceed 10–20 parts per million. As an indication of possible contamination when films are evaporated very rapidly, it should be stated that on one occasion the boat temperature was raised to above 2,000° C. and that the resulting film contained 1 part in 7 of tantalum.

O. S. Heavens

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Measurements of the Normal Stress Effect in Solutions of Poly*iso*butylene

Weissenberg[1–3], Garner and Nissan[4], and Garner, Nissan and Wood[5] have described a number of experiments which indicate that, in high-polymer solutions, steady-state laminar flow cannot be maintained by the action of shearing forces alone, even at low-velocity gradients for which centrifugal effects can be neglected. The effects observed in these experiments have been interpreted by Rivlin[6,7] phenomenologically as due to the fact that the relations connecting the stress and strain velocity components for the steady-state laminar flow of such a solution involve a second physical parameter defining the flow properties of the fluid, in addition to the viscosity.

This parameter has been termed the 'normal stress coefficient', and effects which arise from its non-vanishing have been called 'normal stress effects'. From the phenomenological point of view, both the viscosity and the normal stress coefficient can depend on the state of flow through the flow invariants and, if the flow is inhomogeneous, can vary from point to point of the fluid.

It has further been shown[8] that values of the normal stress coefficient sufficient to give rise to effects of the observed order of magnitude could probably occur in high-polymer solutions of a few per cent concentration as a result of the orientation of the dissolved molecules in the flowing solution.

The normal stress effect for solutions of poly*iso*butylene in *ortho*-dichlorobenzene has now been investigated using an experimental arrangement similar, in principle, to one described by Weissenberg, and by Garner, Nissan and Wood. The solution is contained between two coaxial cylindrical cups, as shown schematically in Fig. 1. The outer cup can be rotated at a variety of speeds and the distance between the disks forming the bases of the cups can be varied. The pressure which must be applied at these disks in order to maintain the solution in a steady state of laminar flow between them is measured automatically by a number of vertical tubes entering the base of the inner cup at various

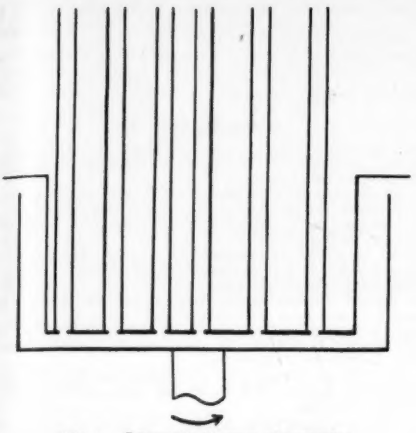

Fig. 1. Schematic diagram of apparatus

radial distances from the axis of rotation. The liquid is drawn up some way into the tubes, for ease of observation of its meniscus, by reducing the pressure in them, and the outer cup is rotated at constant speed. The change in level in each of the vertical tubes is then measured and is proportional to the normal surface traction applied to the plane surface of the fluid at the points of entry of the tubes.

Typical results, obtained with a 6 per cent solution of poly*iso*butylene (viscosity average molecular weight about 2×10^6) in *ortho*-dichlorobenzene, are shown in Fig. 2, in which the height of rise h of the solution in the tubes is plotted against their distance r from the axis of rotation for two different spacings of the disks d, that is, for two mean values of the velocity gradient. It is seen that the relations are substantially linear, small departures from linearity being obtained at low values of r for the higher value of d, and at high values of r for the lower values of d. The former departure from linearity increases and the latter decreases with decrease of the concentration of the solution and decrease of molecular weight of the poly*iso*butylene. It appears that for sufficiently low velocity gradients, concentration or molecular weight, substantial linearity between h and r^2 could be obtained.

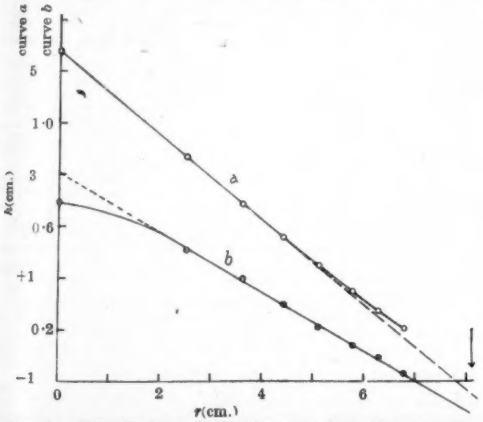

Fig. 2. Normal stress distribution over base of inner cup. h = height of rise of solution in tubes. r = distance of tube from axis of rotation. Curve *a*, disk separation, 0·12 cm.; speed of rotation, 40·9 r.p.m.; curve *b*, disk separation, 1·08 cm.; speed of rotation, 40·9 r.p.m. Arrow on right indicates edge of inner cup

According to the phenomenological theory advanced by Rivlin[6], h is related to r by the formula

$$\rho h = -\tfrac{1}{2}\Psi\frac{\Omega^2}{d^2}r^2 - \tfrac{1}{2}\frac{\Omega^2}{d^2}\int_a^r r\Psi dr, \qquad (1)$$

where ρ is the density of the solution, Ω is the speed of rotation in rad./sec., a is the radius of the cylinder and Ψ is the normal stress coefficient, which depends, in general, on a flow invariant $K_2 = -\frac{1}{4}\Omega^2 r^2/d^2$. This result applies to the ideal case of laminar flow of a cylindrical body of fluid between plane disks, in which the curved surface of the fluid remains free. In the experimental arrangement used, the curved surface of the fluid is not free and therefore the absolute value of h, as distinct from its variation with r, will depend on the conditions prevailing at the edge of the disks.

Neglecting the small departures from a linear relationship between r and h found experimentally, we see that Ψ must be proportional to $(-K_2)^{-1/2}$ over most of the range of velocity gradients covered by the experiments, so that

$$\rho(h - h_0) = \alpha\frac{\Omega}{d}(a - 2r), \qquad (2)$$

where h_0 is a constant depending on the state of flow at the edge of the disks and α is given by $\Psi = \alpha(-K_2)^{-1/2}$. Mooney[9] has reported that results similar to (2) have been obtained by DeWitt with solutions of 'Vistanex' (poly*iso*butylene) in tetralin.

The particular relation between Ψ and K_2 leading to equation (2) is indicated by the linear relationship between r and h. It is seen that it implies a linear relationship between h and Ω/d. This has been verified experimentally.

Experiments carried out over the concentration-range 2·5–7·8 per cent showed that α is roughly proportional to the fourth power of the concentration. It has also been found that α decreases with increase of temperature. For example, for a 6 per cent solution, α decreased approximately linearly to about half its original value as the temperature was increased from 10° to 37° C.

A detailed account of the results obtained, together with the experimental precautions which were found necessary to ensure reproducibility and significance for the results, will be published later. A more critical discussion of their relationship to the various theories which have been advanced will then be given.

The work described in this communication forms part of a programme of fundamental research undertaken by the Board of the British Rubber Producers' Research Association and was carried out in the Davy Faraday Laboratory of the Royal Institution.

H. W. Greensmith
R. S. Rivlin

Davy Faraday Laboratory,
Royal Institution,
London, W.1. June 5.

[1] Weissenberg, K., Conf. British Rheologists' Club, London, 1946.
[2] Weissenberg, K., *Nature*, **159**, 310 (1947).
[3] Weissenberg, K., *Proc. Roy. Soc.*, A, **200**, 183 (1950).
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[7] Rivlin, R. S., *Proc. Roy. Soc.*, A, **200**, 168 (1950).
[8] Rivlin, R. S., *Trans. Farad. Soc.*, **45**, 739 (1949).
[9] Mooney, M., *J. Colloid Sci.*, **6**, 96 (1951).

FORTHCOMING EVENTS

*(Meetings marked with an asterisk * are open to the public)*

Monday, October 15

University of London (at the Institute of Neurology, National Hospital, Queen Square, London, W.C.1), at 5 p.m.—Dr. Donald Hunter: "Poisoning by Fluorine Compounds" (Guest Lecture).*

University of London (at the London School of Hygiene and Tropical Medicine, Keppel Street, London, W.C.1), at 5.15 p.m.—Sir Philip Manson-Bahr: "Patrick Manson, the Father of Tropical Medicine (his Pioneer Work and its Influence on Modern Advances)" (Manson Lecture).*

British Society for the History of Science, Philosophy of Science Group (in the Council Room, University College, Gower Street, London, W.C.1), at 5.30 p.m.—Mr. W. Kneale: "Observables and Unobservables".

Manchester Literary and Philosophical Society (in the Reynolds Hall, College of Technology, Manchester), at 5.30 p.m.—Mr. H. Hayhurst: "Infestation in Stored Products" (Presidential Address).*

Institution of Electrical Engineers (at Savoy Place, Victoria Embankment, London, W.C.2), at 7 p.m.—Mr. K. C. Pounds: "Keeping Electricity Safe".

Tuesday, October 16

Society of Chemical Industry, Agriculture Group (in the Chemistry Lecture Theatre, Royal College of Science, Imperial Institute Road, London, S.W.7), at 2.30 p.m.—Dr. E. W. Russell: "Soil Structure".*

Institute of Fuel (at the Institution of Mechanical Engineers, Storey's Gate, St. James's Park, London, S.W.1), at 5.30 p.m.—Prof. F. H. Garner: "Combustion Processes in Engines" (Melchett Lecture).*

University of London (at the London School of Hygiene and Tropical Medicine, Keppel Street, London, W.C.1), at 5.30 p.m.—Sir Jack Drummond, F.R.S.: "The Scientific Basis of Medicine, 3, Practical Aspects of Modern Nutritional Theories".* (Further Lectures on October 18, 23, 25 and 30, November 2, 5, 7, 14, 16, 20, 23, 27 and 29, December 4, 6 and 11.)

Institution of Electrical Engineers, Education Discussion Circle (at Savoy Place, Victoria Embankment, London, W.C.2), at 6 p.m.—Discussion on "The 'j' Operator Method of Calculation—When and How should it be Introduced ?" (to be opened by Mr. A. W. Gillies).

Textile Institute, Scottish Section (joint meeting with the Scottish Section of the Institution of the Rubber Industry, at 25 Charlotte Square, Edinburgh), at 7.30 p.m.—Mr. G. Appleyard: "Textile Development in the Rubber Industry".

Textile Institute, Scottish Section (joint meeting with the Institution of the Rubber Industry and Plastics Institute, at the Royal Technical College, Glasgow), at 7.30 p.m.—Mr. A. M. Dobson: "Glass Textiles as Reinforcement in the Rubber and Plastics Industries".

Wednesday, October 17

Institute of Metals (at Park Lane Hotel, Piccadilly, London, W.1), at 10 a.m.—General Discussion on "Metal Economics".

Geological Society of London (at Burlington House, Piccadilly, London, W.1), at 5 p.m.—William Smith Lecture.

University of London (at the London School of Economics and Political Science, Houghton Street, Aldwych, London, W.C.2), at 5 p.m.—Prof. G. Friedmann: "Technological Change and Human Relations".* (Further Lecture on October 19.)

Manchester Statistical Society (at the Reform Club, King Street, Manchester), at 5.30 p.m.—Mr. J. Stafford: "Census of Production".

Society of Chemical Industry (joint meeting of the Food and Microbiology Groups with the London and South-Eastern Counties Section of the Royal Institute of Chemistry, at the Chemical Society, Burlington House, Piccadilly, London, W.1), at 6.30 p.m.—Dr. J. R. Vickery: "Bacterial Wastage in Eggs and its Control".

Thursday, October 18

University of London (in the Anatomy Theatre, University College, Gower Street, London, W.C.1), at 1.15 p.m.—Dr. M. K. Wells: "Exploring the Roots of Extinct Volcanoes".*

Institution of Mining and Metallurgy (at the Geological Society of London, Burlington House, Piccadilly, London, W.1), at 5 p.m.—Scientific Papers.

University of London (at the London School of Hygiene and Tropical Medicine, Keppel Street, London, W.C.1), at 5.30 p.m.—Sir Jack Drummond, F.R.S.: "The Scientific Basis of Medicine, 4, Practical Aspects of Modern Nutritional Theories".* (Further Lectures on October 23, 25 and 30, November 2, 5, 7, 14, 16, 20, 23, 27 and 29, December 4, 6 and 11.)

Society of Chemical Industry, Road and Building Materials Group (at the Institution of Structural Engineers, 11 Upper Belgrave Street, London, S.W.1), at 6 p.m.—Dr. A. R. Lee and Mr. E. J. Dickinson: "The Durability of Tar and Tarmacadam".

Chemical Society (joint meeting with the Local Sections of the Royal Institute of Chemistry and the Society of Chemical Industry, at The University, Manchester), at 6.30 p.m.—Prof. A. R. Ubbelohde, F.R.S.: "Some Problems in the Oxidation of Hydrocarbons".

Society of Public Analysts and Other Analytical Chemists, Microchemistry Group (joint meeting with the Liverpool and North-Western Section of the Royal Institute of Chemistry, in the Chemistry Lecture Theatre, The University, Brownlow Street, Liverpool), at 7 p.m.—Discussion on "Chemical Microscopy".

Chemical Society (at Burlington House, Piccadilly, London, W.1), at 7.30 p.m.—Prof. C. A. Coulson, F.R.S.: "The Contributions of Wave Mechanics to Chemistry" (Tilden Lecture).

Royal Society of Tropical Medicine and Hygiene (at Manson House, 26 Portland Place, London, W.1), at 7.30 p.m.—Sir Neil Hamilton Fairley, F.R.S.: "Schistosomiasis and some of its Problems" (Presidential Address.)

Friday, October 19

Royal Sanitary Institute (in the Council Chamber, Town Hall, Stockport), at 10 a.m.—Papers on "The Prevention and Control of Infectious Diseases".

British Mycological Society (in the Main Chemistry Lecture Theatre, Imperial College of Science and Technology, Imperial Institute Road, London, S.W.7), at 10.30 a.m.—Discussion on "Epidemiology of Air-borne Fungus Diseases".

Textile Institute, Lancashire Section (at 16 St. Mary's Parsonage, Manchester), at 1 p.m.—Sir Robert R. Hyde: "Changes and Developments in Industrial Relations".

Biochemical Society (at the London Hospital Medical College, London, E.1), at 2 p.m.—Scientific Papers.

Royal Institute of Philosophy (at University Hall, 14 Gordon Square, London, W.C.1), at 5.15 p.m.—Prof. H. Dingle: "Science".* (Further Lecture on October 26.)

Institution of Mechanical Engineers (at Storey's Gate, St. James's Park, London, S.W.1), at 5.30 p.m.—Mr. A. C. Hartley: Presidential Address.

Manchester Statistical Society, Industrial Study Group (at the Albert Hall, Peter Street, Manchester), at 6.45 p.m.—Mr. P. J. Stanley: "Some Aspects of Production Statistics in the Aircraft Industry".

Society of Public Analysts and Other Analytical Chemists, Physical Methods Group (joint meeting with the Tees-side Local Section of the Royal Institute of Chemistry, at the William Newton School, Norton, Stockton-on-Tees), at 7 p.m.—Papers on "Physical Methods in the Pigment Industry".

Saturday, October 20

Botanical Society of the British Isles (at the Natural History Museum, Cromwell Road, London, S.W.7), at 2.30 p.m.—Exhibition Meeting.

British Psychological Society (in the Eugenics Theatre, University College, Gower Street, London, W.C.1), at 2.30 p.m.—Prof. T. M. Newcomb: "Some Research on the Concept of Reference Groups".

Institution of Chemical Engineers, North-Western Branch (in the Reynolds Hall, College of Technology, Manchester), at 3 p.m.—Sir Harold Hartley, F.R.S.: "Chemical Engineering and the Future".

London County Council (at the Horniman Museum, London Road, Forest Hill, London, S.E.23), at 3.30 p.m.—Dr. W. E. Swinton: "100 Years of English Vertebrate Palæontology".*

APPOINTMENTS VACANT

Applications are invited for the following appointments on or before the dates mentioned:

Assistant Lecturers for Biology and Chemistry—The Clerk to the Governors, Essex Institute of Agriculture, Writtle, Chelmsford (October 20).

General Assistant Engineer (Mathematician), and a Third Assistant Engineer (Mathematician), in the research branch of the Chief Engineer's Department in London—The Director of Establishments, British Electricity Authority, British Electricity House, Great Portland Street, London, W.1, quoting Ref. No. AE/206 (October 20).

Research Assistant to work on problems of applied economics forming part of the research programme of the Department of Economics—The Registrar, University College, Hull (October 20).

Scientific Officer (with a University degree in botany or agricultural botany, with a sound knowledge of the principles of genetics and practical plant breeding) in the Plant Breeding Division of the Ministry of Agriculture—The Director of Establishments, Ministry of Finance, Stormont, Belfast (October 22).

Statistician (with thorough knowledge of statistical method, preferably in relation to social research) in the Sociological Research Unit—The Secretary, London School of Economics and Political Science, Houghton Street, Aldwych, London, W.C.2 (October 22).

Research Fellow in Economics or Economic History at the East African Institute of Social Research, Makerere College (University College of East Africa)—The Secretary, Inter-University Council for Higher Education in the Colonies, 1 Gordon Square, London, W.C.1 (October 25).

Demonstrator in the Department of Aircraft Propulsion—The Registrar, College of Aeronautics, Cranfield, Bletchley, Bucks, endorsed 'Demonstrator, Aircraft Propulsion' (October 27).

Chair of Organic Chemistry at the University of Sydney—The Secretary, Association of Universities of the British Commonwealth, 5 Gordon Square, London, W.C.1 (November 12).

Lecturer (with a University degree or diploma, either in engineering or in agriculture) in Agricultural Engineering at Canterbury Agricultural College (University of New Zealand)—The Secretary, Association of Universities of the British Commonwealth, 5 Gordon Square, London, W.C.1 (November 12).

Agronomist in the Department of Agriculture, Gold Coast—The Director of Recruitment (Colonial Service), Colonial Office, Sanctuary Buildings, Great Smith Street, London, S.W.1, quoting Ref. No. 27059/249.